Essays on the Economics of Structural Change

Evidence from the Division and Reunification of Germany

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Abstract

In the context of structural change, how do economic factors impact socio-economic outcomes? In this thesis, I leverage the division and reunification of Germany to investigate two specific topics pertaining to this fundamental question.

In the first essay, I analyze how a negative labor demand shock impacts fertility. I analyze this question in the context of the East German fertility decline after the fall of the Berlin Wall in 1989. I exploit differential pressure for restructuring across East German industries which led to unexpected, exogenous, and permanent changes to labor demand. I find that throughout the 1990s, women more severely impacted by the demand shock had relatively more children than their less-severely-impacted counterparts. Thus, the demand shock not only depressed the aggregate fertility level, but also changed the composition of mothers. My paper shows that these two effects do not necessarily operate in the same direction.

The second essay of this thesis explores the question of how refugee-specific aid impacts the medium-term economic success of refugees who migrate as children and young adults. We address this question in the historical context of German Democratic Republic (GDR) refugees who escaped to West Germany between 1946 and 1961, exploiting the facts that only the subgroup of "political refugees" was granted refugee-targeted aid, and that this only occurred after 1953. The quasi-experiment allows us to combine several approaches to address identification difficulties resulting from the fact that refugees eligible for aid are both self-selected and screened by local authorities. We find positive effects of aid-eligibility on educational attainment, job quality and income among the refugees who migrated as young adults. We do not find similar effects of aid-eligibility for refugees who migrated as children.

The final chapter of this thesis presents results of a project which partially closes a gap that currently exists for East Germans in the data from German social security notifications and the internal procedures of the Federal Employment Agency. By linking these data with the GDR's "Data Fund of Societal Work Power" from 1989, we have created a new data set that permits the analysis of phenomena such as unemployment, job mobility, and regional mobility. The new data set can also be used to refine existing knowledge of the individual-level labor market consequences of German reunification.

Keywords:

Fertility, Labor Demand Shock, Industrial Restructuring;
Refugees, Government Aid, Economic Success;
Labor Market Trajectories, Administrative Data, Record Linkage;
Division and Reunification of Germany, Structural Change

Zusammenfassung

Wie wirken sich, im Kontext strukturellen Wandels, ökonomische Einflussfaktoren auf sozio-ökonomische Ergebnisse aus? Basierend auf der Teilung und Wiedervereinigung Deutschlands, untersuche ich in dieser Dissertation zwei spezifische Themen, die sich auf diese fundamentale Frage beziehen.

Im ersten Aufsatz analysiere ich, wie sich ein negativer Arbeitsmarktnachfrage-Schock auf Fertilität auswirkt. Ich analysiere diese Frage im Kontext des ostdeutschen Fertilitätsrückgangs nach dem Fall der Berliner Mauer im Jahr 1989. Meine empirische Strategie stützt sich auf unterschiedliche Grade der Restrukturierung auf Industrie-Ebene, welche unerwartete, exogene, und permanente Anpassungen der Arbeitsnachfrage zur Folge hatten. Ich zeige, dass in den 1990er Jahren ostdeutsche Frauen, welche stärker vom negativen Arbeitsnachfrage-Schock betroffen waren, relativ mehr Kinder bekommen haben als Frauen, die weniger stark von dem Schock betroffen waren. Dies impliziert, dass der Arbeitsnachfrage-Schock nicht nur das aggregierte Fertilitätsniveau gesenkt hat, sondern auch Auswirkungen auf die Zusammensetzung (Komposition) der Mütter hatte. Meine Analyse zeigt, dass diese beiden Effekte nicht notwendigerweise in die gleiche Richtung wirken.

Der zweite Aufsatz dieser Dissertation betrifft die Frage, wie sich Flüchtlings-spezifische staatliche Hilfen auf den mittelfristigen ökonomischen Erfolg von Flüchtlingen, die als Kinder und junge Erwachsene geflohen sind, auswirken. Wir untersuchen diese Frage im historischen Kontext ostdeutscher Flüchtlinge, die zwischen 1946 und 1961 aus der Deutschen Demokratischen Republik (DDR) nach Westdeutschland geflohen sind; und nutzen empirisch aus, dass nur der Untergruppe "politischer Flüchtlinge" Anspruch auf Flüchtlings-spezifische Hilfen gewährt wurde sowie lediglich erst ab dem Jahr 1953. Dieses Quasi-Experiment ermöglicht es uns, verschiedene Ansätze zu verfolgen, welche Identifikations-Probleme adressieren, die daraus resultieren, dass für Flüchtlingshilfen berechtigte Personen sowohl selbst-selektiert waren als auch von den Behörden ausgewählt wurden. Im Ergebnis zeigen sich positive Effekte der Flüchtlings-spezifischen Hilfen auf die Bildungsabschlüsse, die Qualität der Jobs, und auf das Einkommensniveau von Flüchtlingen, die als junge Erwachsene migriert sind. Wir finden keine vergleichbaren Ergebnisse für Flüchtlinge, die als Kinder migriert sind.

Das letzte Kapitel dieser Dissertation präsentiert Ergebnisse eines Projektes, das partiell die Lücke schließt, welche derzeit für Ostdeutsche in den Daten der deutschen Sozialversicherung sowie der internen Datenerfassung der Bundesagentur für Arbeit existiert. Durch die Verknüpfung dieser Daten mit den Daten des "Datenspeichers Gesellschaftliches Arbeitsvermögen" der DDR aus dem Jahr 1989 haben wir einen neuen Datensatz geschaffen, welcher Analysen von Phänomenen wie Arbeitslosigkeit, berufliche Mobilität, und regionale Mobilität ermöglicht. Der neue Datensatz kann auch dazu

beitragen, das existierende Wissen über die individuellen Arbeitsmarktkonsequenzen des Mauerfalls zu erweitern.

Schlagwörter:
Fertilität, Arbeitsnachfrage-Schock, Industrielle Restrukturierung;
Flüchtlinge, Staatliche Hilfen, Ökonomischer Erfolg;
Berufliche Verläufe, Administrative Daten, Datenverknüpfung (Record Linkage);
Teilung und Wiedervereinigung Deutschlands, Strukturwandel

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During my doctoral studies I had the opportunity to spend five months at the Institute for Research on Labor and Employment at UC Berkeley. I thank Jesse Rothstein and Daniel Schneider for making this stay possible and for their extremely helpful comments and suggestions.

I learned a lot from working with Sandra E. Black, Camille Remigereau, and Alexandra Spitz-Oener on our joint project that is presented in Chapter 3 of this thesis. To obtain access to the data used in this project, I went to Mannheim for several short research visits at the GESIS Leibniz Institute for the Social Sciences, where Bernhard Schimpl-Neimanns has been very supportive.

As part of my doctoral studies, I also spent time at the Institute for Employment Research (IAB) in Nuremberg. I thank Dana Müller for her hospitality during this time. Our stimulating collaboration has led to Chapter 4 of this thesis. I also thank Manfred Antoni of the IAB for interesting discussions.

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I also thank Eileen Appelbaum and Heidi Hartmann for their encouraging advice and generous support. They are inspiring role models for me. Knut Gerlach was kind enough to provide detailed comments on Chapter 2 of this thesis, when it was still at an early stage. Flora Budianto and I have supported each other ever since we started our graduate program together. I am also grateful to Ariane Hegewisch who has been an important mentor even before and also after I started this dissertation.

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1 Introduction

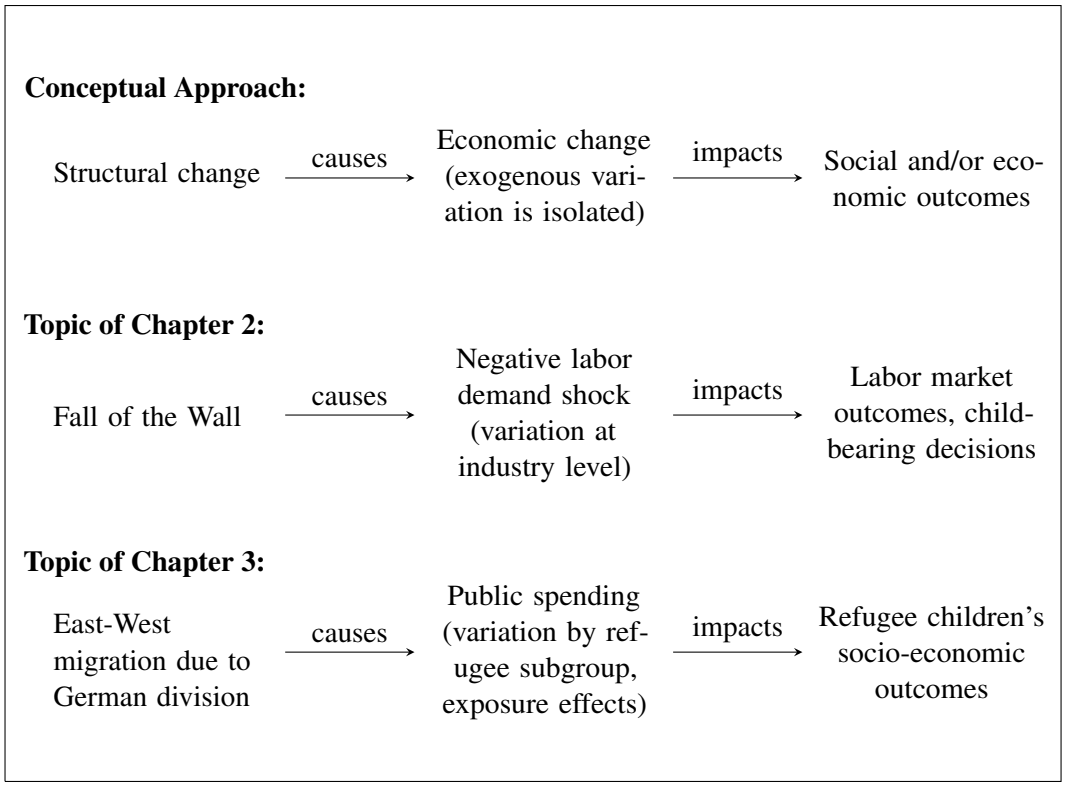
What are the consequences of structural change? And, in the context of structural change, how do economic factors impact socioeconomic outcomes? In this thesis, I approach these fundamental questions by exploring two particular issues in the context of a stark period of structural change: the division and reunification of Germany. I leverage this historical context to investigate the causal nexus between economic factors and socio-economic outcomes. Specifically, I analyze, first, how women's labor market situations affect their childbearing decisions; and second, the impact of government aid on refugee children's medium-term economic success.

Of course, structural change is a broadly defined phenomenon that can refer to pronounced adjustments in the economic sphere; or that is initially caused by substantial shifts in the social and political arenas. The interdependencies between these aspects of society imply that the consequences of structural change will reverberate throughout society, affecting the lives of the population in myriad ways.

The examples of the German division and reunification clearly illustrate this point: The fall of the Berlin Wall in 1989, and the subsequent collapse of communism, profoundly and permanently changed the lives of around 16 million East Germans (see for instance Huinink and Mayer, eds., 1995; Mayer and Solga, 2010). Due to the introduction of democracy and the market economy, citizens of the former German Democratic Republic (GDR) had to adapt to a new systemic order, which brought about new basic freedoms. At the same time, their society experienced a severe economic crisis (Akerlof et al., 1991; Burda and Hunt, 2001).

Structural change was similarly complex before 1961, when the planned economy was introduced in East Germany. Aiming to achieve a more egalitarian society, the totalitarian regime in East Germany focused on political oppression and severe economic restrictions and disappropriations that targeted, in particular, those citizens who formerly enjoyed a privileged socio-economic status. As a result, before the Berlin Wall was built in 1961, at least 3.6 million East Germans escaped to West Germany, where they would need time to become integrated into society and the labor market (Bethlehem, 1982; Heidemeyer, 1994; Ackermann, 1995; Van Melis, 2006).

Figure 1.1: Conceptual Approach and Specific Research Topics



Because of the complexity of structural change, I pay careful attention to isolating exogenous variation in economic factors. In the context of my two specific research questions, the methodological approach allows me to assess the causal nexus between economic factors and socio-economic outcomes (see Figure 1.1 for a summary). In this sense, my thesis is methodologically inspired by previous studies that have interpreted the same historical frame as a quasi-experiment in order to uncover causal effects. These studies have focused on a range of causal relationships regarding phenomena such as solidarity (Ockenfels and Weimann, 1999), occupational choice (Fuchs-Schündeln and Schündeln, 2005), preferences for redistribution (Alesina and Fuchs-Schündeln, 2007), saving behavior (Fuchs-Schündeln, 2008), market access (Redding and Sturm, 2008), occupational entry regulation (Prantl and Spitz-Oener, 2009), social networks (Burchardi and Hassan, 2013), migration (Prantl and Spitz-Oener, 2014), city structures (Ahlfeldt et al., 2015), consumption (Bursztyn and Cantoni, 2016), and gender norms (Beblo and Görges, 2018).

Figure 1.1 summarizes the conceptual approach that I employ as well as the specific topics that I analyze. In Chapter 2, I revisit the question of how women's labor market situations impact childbearing decisions. I analyze the question in the context of the dramatic

decline in aggregate East German fertility after the fall of the Berlin Wall. To circumvent the endogeneity of women's labor market situations, I exploit variation in the labor demand shock which hit East Germany during this time. The variation stems from differential pressure for restructuring across industries. This empirical strategy is particularly suited for the purposes of my research question, since industrial restructuring in East Germany was unexpected and therefore exogenous to prior selection into industries. Furthermore, industrial restructuring led to permanent changes to women's labor market situations.

My paper makes two contributions to the literature. First, I build on previous evidence by Chevalier and Marie (2017) on the East German fertility decline, but in contrast to these authors, I exploit exogenous variation in women's labor market situations and assess how these impact childbearing decisions. Second, my study is related to Schaller (2016) and Autor et al. (2018) who also exploit industry-level variation of demand shocks. Whereas the level of analysis in these two studies is the regional level, I analyze fertility at the individual level and follow selected cohorts of women over time. I argue that it is important to understand such individual-level mechanisms, as they have implications for women's life courses and labor market trajectories. These mechanisms can also have implications for children to the extent that parent's labor market situations are associated with an intergenerational transmission of inequalities (see the review by Brand, 2015).

My analysis is based on rich panel data from German unemployment and pension insurance records, called BASiD. I find that the demand shock impacted the composition of mothers: Throughout the 1990s women more severely impacted by the demand shock had relatively more children than their less-severely impacted counterparts and this effect persisted over time. My results thus point to a trade-off between female careers and childbearing. In the uncertain economic environment in East Germany after reunification, women with more favorable labor market situations had relatively fewer children because they apparently were less willing to put their jobs and labor market prospects at risk. More generally, I show that demand shocks not only depress aggregate fertility levels but also change the composition of mothers. Indeed, my study demonstrates that, perhaps surprisingly, level and composition effects of labor demand shocks on fertility do not necessarily operate in the same direction. Therefore, composition effects should also be investigated in other contexts, particularly because they entail consequences of economic and social relevance for women and their families.

Chapter 3 of this thesis is jointly written with Sandra E. Black, Camille Remigereau, and Alexandra Spitz-Oener. It analyses the impact of refugee-specific government aid on child

refugees' economic success later in life (see Figure 1.1). The chapter is motivated by the fact that there is surprisingly little research on how welfare affects the success of refugees, with the notable exceptions of Andersen et al. (2018) and LoPalo (2018).

We focus on refugees escaping from the GDR to West Germany between 1946 and 1961 and examine a range of economic outcomes measured 10 years after the migration stream ceased. The GDR refugees were not welcomed with open arms, leading to a situation in which merely the subgroup acknowledged as genuine "political refugees" was eligible for refugee-specific aid (and only from 1953 onward). This allows us to compare refugees eligible for aid with their non-eligible counterparts. Using rich Microcensus data collected in West Germany in 1971, we are able to control for detailed parental background characteristics that the authorities relied on when distinguishing aid-eligible from non-eligible individuals. Furthermore, we exploit variation in years of exposure to aid. We find that refugee-specific government aid had an economically significant impact on the educational attainment, job quality, and income among those refugees who migrated as young adults. In contrast, we do not find similar effects of aid eligibility for refugees who migrated as children.

Our findings demonstrate the importance of age-at-arrival and the institutional link to the host country. In the medium-term, non-eligible refugees migrating as children were able to catch up with their aid-eligible counterparts. We argue that this was due to their natural integration in the host country's educational institutions, and emphasize that the refugee-specific aid was granted in addition to the comprehensive West German social welfare net that all GDR refugees were covered by. However, refugees arriving as young adults were vulnerable to a lack of refugee-specific aid. Faced with the decision of entering the labor market and earning a wage immediately, and given their families' severe liquidity constraints, young adults lacking refugee-specific aid were not able to invest in higher education and suffered the economic consequences this entailed in the medium term. Our findings thus point to a need for policy makers to consider the specific circumstances of young refugees who are above the compulsory schooling age.

The final Chapter 4 is coauthored with Dana Müller. It is a methodological report that relates to Chapter 2. An empirical challenge that I had to tackle in Chapter 2 was the issue that for East Germans, data from social security records are only fully available from 1992 onward.[1] This is due to the fact that the East German labor market administration was integrated into the West German administration through a complex process. It took time before all East German firms reported to the social security system (Schmid and Oschmiansky,

[1] In Chapter 2, I worked with a sample for which earlier information was available and constructed weights to make the sample representative by industry. See Section 2.3.2 below for details.

2007). By 1992, considerable fractions of East Germans had already lost their jobs, had changed their occupations and industries, or had moved to West Germany. A large number of firms had closed (see e.g., Diewald et al., 1995; Burda and Hunt, 2001; Hunt, 2006). For many research questions, 1992 is thus too late in time as a starting point for analysis.

The report presented in Chapter 4 provides results from a project that partially closes the gap for East Germans in the social security data. From the Federal Archive of Germany we obtained a cross-section of the GDR's "Data Fund of Societal Work Power" (for previous descriptions of these data see Solomon, 1981; Dietz and Rudolph, 1990; Rathje, 1996; Gebauer et al., 2004). The cross-section contains information on around 7 million East German workers in 1989, which amounts to 72 percent of the East German labor force at that time. Based on names, exact dates of birth, and gender information, we merged the 1989 data with data from social security records that start in 1992. In Chapter 4 we describe the original data sources that we merged, provide details on the merging procedure, and evaluate its quality. The evaluation shows that we were able to obtain a comparatively high merging quota of 82 percent for workers younger than 60 in 1989. It furthermore reveals that the merge was less successful for older workers, who dropped out of the labor force, or initially single women, who often changed their names after marriage.

Overall, we have created a unique and promising new data set that has two major advantages. First, it allows researchers to study mechanisms behind phenomena of general relevance, such as unemployment, occupational mobility, mobility across industries, and regional mobility. Second, it permits the analysis of East German labor market trajectories around reunification based on a sample size that is considerably larger than the sample sizes of currently existing data sources with panel structure. This is particularly relevant because differences between the former East and West Germany remain an important dimension of the persistent socio-economic disparities in Germany. This implies a need for further scientific investigations that are thorough and transparent (recent evidence on the need for further research is provided in Goschler and Böick, 2017).

To summarize, the three chapters of this thesis are self-contained and can be read independently, but they all address topics that pertain to the consequences of structural change induced by the division and reunification of Germany. Of course, this context influences how the empirical results are interpreted. At the same time, the context is particularly appropriate for empirically exploiting exogenous variation in economic factors. Therefore, this thesis refines the existing knowledge on two specific questions that concern the causal nexus between economic factors and socio-economic outcomes: the impact of women's

labor market situations on childbearing decisions as well as the effect of refugee-specific government aid on child refugees' economic success later in life.

2 The Impact of a Negative Labor Demand Shock on Fertility

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with DOI: https://doi.org/10.1016/j.labeco.2018.07.003.[1]

2.1 Introduction

In this paper, I revisit the question of how women's labor market situations impact childbearing decisions. From a neoclassical point of view, this is an empirical question, as there are opposing income and substitution effects (e.g., Becker, 1965; Gronau, 1977). Yet the endogeneity of individuals' labor market outcomes poses a key empirical challenge. I analyze the question in the context of East Germany after the fall of the Berlin Wall in 1989. Fertility in the formerly communist country plummeted after 1989 and recovered only slowly in later years (Figure 2.1).[2] The magnitude of this decline in East German fertility is unprecedented (Eberstadt, 1994). It stands in contrast to the relatively constant and already low West German fertility level.

The East German setting is particularly suited to study the effect of women's labor market prospects on fertility. To tackle the endogeneity problem, I exploit exogenous variation in the negative labor demand shock which hit East Germany as a consequence of the introduction of the market economy. The variation stems from differential pressure for restructuring across East German industries. A unique advantage of my empirical strategy results from the unexpectedness of the demand shock. In the former German Democratic Republic self-selection into industries was independent of later industrial restructuring. Instead, prior to German reunification, East German workers were accustomed to remarkably stable industrial

[1] The thesis chapter and Liepmann (2018) are identical, with the exception of minor editorial adjustments.

[2] The decline was due to postponement of childbearing (Conrad et al., 1996; Lechner, 2001), but completed fertility also declined (Kreyenfeld, 2003; Goldstein and Kreyenfeld, 2011).

Figure 2.1: Total Fertility Rates by Region and Year, 1980-2012

Source: Human Fertility Database, Max Planck Institute for Demographic Research (Germany) and Vienna Institute of Demography (Austria), available at www.humanfertility.org (data downloaded in March, 2015). See Goldstein and Kreyenfeld (2011, p. 454) for a similar graph. The total fertility rates are defined for each year as the unweighted sum of all age-specific birth rates for women in their childbearing years.

employment structures, full employment guaranteed by the state, and constrained job choice under central planning.

The nature of the labor demand shock represents another distinguishable feature of the East German setting. The shock is a one-time event which led to sharp and permanent structural changes to labor demand. This allows me to isolate how the effects of the shock evolved over time. Specifically, I show that in 1989, the East German employment distribution over industries strongly differed from economic structures in market economies. Within only a few years after reunification East German industrial employment structures converged to the market economy benchmark provided by West Germany. I exploit this fact to abstract from endogenous supply-side adjustments and to measure the varying intensity of the negative labor demand shock. The East German setting is an interesting test case, because the rapid shifts in East German industrial employment structures resemble changes to industrial employment structures in market economies. In market economies, these changes were considerably more gradual, but they likewise implied the decline of the manufacturing (Autor et al., 2003, 2013) and the rise of the services sector (Autor and Dorn, 2013).

My analysis is based on rich administrative data from German unemployment and pension insurance records, called BASiD. The panel structure of these data permits a detailed

individual-level analysis of fertility over a relatively long time period of seventeen years. Moreover, in the BASiD data I am able to identify the significant fraction of East German women who migrated to West Germany. This allows me to study childbearing decisions of East German women rather than of women living in East Germany.

To preview the results, I first establish that the industry-level labor demand shock generated exogenous variation in individuals' labor market outcomes by increasing unemployment and inducing mobility across industries. I then show that this had an impact on the composition of mothers: Throughout the 1990s, women more severely impacted by the labor demand shock had relatively more children than their counterparts who were less severely impacted. This composition effect is economically significant and it persists over the seventeen year period. The composition effect is moreover robust to evaluating the influence of migration to West Germany, qualification levels, child care, regional spill over effects, firm-level characteristics, and the presence of assortative mating. Furthermore, the composition effect is robust when older cohorts of East German women are used as a control group. Finally, my empirical estimates suggest a small permanent effect on completed fertility.

My results point to a trade-off between female careers and childbearing: In the uncertain economic environment in East Germany after reunification, women with more favorable labor market outcomes had relatively fewer children because they apparently were less willing to put their current jobs and future labor market prospects at risk. This mechanism affected the composition of mothers against the backdrop of a low aggregate fertility level. In terms of neoclassical theory, the substitution effect dominated over the income effect in determining the composition of mothers. The results suggest that the compatibility between female careers and childbearing is crucial. During downturns in particular, the availability of high-quality childcare might, for example, help avoiding a higher birth reduction among women with relatively better labor market prospects.

As a general implication of my results, it is important to distinguish between the level effect and the composition effect of a labor demand shock on fertility. It is a stylized fact that the aggregate fertility level tends to decline in industrialized countries during recessions (Sobotka et al., 2011). In East Germany, this negative level effect was very pronounced and aggregate fertility declined strongly for all groups of East German women (see Figure 2.1). The negative level effect was due to rapid systemic change after German reunification (Frejka, 2008). While the level effect was not caused by a single factor alone, a major contributing factor was the aggregate negative labor demand shock. Many researchers have therefore attributed the fertility decline to economic uncertainty.[3] However, contrarily to

[3] In particular, Chevalier and Marie (2017) emphasize the importance of elevated economic uncertainty in causing the decline in aggregate fertility (see also Eberstadt, 1994; Conrad et al., 1996; Sobotka et al., 2011).

what one might expect intuitively, my paper shows that this does not imply that those women most severely impacted by the demand shock had the lowest birth rates. On the contrary, women more severely impacted by the demand shock had relatively more children than their less-severely-impacted counterparts.[4] Therefore, the level and composition effect of the labor demand shock did not operate in the same direction. It is generally important to understand such composition effects. These individual-level dynamics have implications for the life courses and labor market trajectories of women. They can also have long-term consequences for the next generation, since parents' labor market outcomes affect children to the extent that socio-economic inequalities persist across generations.

My paper is related to three strands of literature. First, I build on previous studies on the East German fertility decline. Chevalier and Marie (2017) find that East German women who had children after the fall of the Berlin Wall were negatively selected in terms of observable characteristics. They show that these selection effects caused poor educational outcomes of East Germans born during this time. Moreover, these authors stress feelings of economic uncertainty as a determinant for childbearing decisions after reunification. In particular, the authors demonstrate that East German women who had reported to be highly worried about the economy were less likely to subsequently have children. My results highlight another mechanism explaining the selection into motherhood. In contrast to Chevalier and Marie (2017), I exploit exogenous variation in women's employment situations and show that women more severely impacted by the labor demand shock had relatively more children than their less-severely-impacted counterparts. This result can potentially give an additional explanation why Chevalier and Marie (2017) find worse outcomes for children born after reunification. Parents' labor market outcomes affect children to the extent that socio-economic inequalities persist across generations (see the review by Brand, 2015). For married couples in Norway, Rege et al. (2011) find negative effects of paternal but not maternal job loss on children's school performance. However, maternal job loss is detrimental for children in contexts where maternal employment and female breadwinner roles are more normative (Brand and Simon Tomas, 2014). Given the high labor force attachment of East German women (Rosenfeld et al., 2004; Klenner et al., 2012), women's labor market situations presumably entailed intergenerational spill-over effects also in the East German context after reunification.

Other studies on the East German fertility decline include Arntz and Gathmann (2014) who

In contrast, Arntz and Gathmann (2014) stress the importance of new opportunities. In my view, these two explanations are not mutually exclusive.

[4] Earlier research shows that aggregate economic conditions lead to such composition effects (Dehija and Lleras-Muney, 2004; Chevalier and Marie, 2017); whereas I focus on the causal impact of individuals' labor market outcomes.

focus on returns to experience in market economies and find that predicted motherhood wage penalties led to lower birth rates among East German women. Bhaumik and Nugent (2011) and Kreyenfeld (2010) document a negative impact of perceived employment uncertainty on birth rates of East German women. Women's perceived employment uncertainty of partners (Bhaumik and Nugent, 2011) or actual unemployment (Kreyenfeld, 2010) had no impact. Finally, in accordance with my findings, Kohler and Kohler (2002) show that in Russia during the mid 1990s, less favorable labor market outcomes were in several cases positively correlated with fertility.

Second, this paper is related to two previous studies which exploit industry-level variation of changes to labor demand in the United States.[5] Schaller (2016) uses a Bartik-type instrumental variable strategy. Autor et al. (2018) focus on import competition from China.[6] With regard to fertility, both studies find that fertility tends to increase in regions where female labor market prospects decline. My contribution to this literature is twofold. To begin with, compared to the shift-share approaches employed in these previous studies, my measure for the demand shock exploits variation at the industry level but not based on geographic differences in initial industry concentration. Thus, my approach is not affected by serial correlation which is a potential concern in the context of shift-share analysis. In addition, the level of analysis in both previous studies is the regional level, whereas I investigate individual-level fertility. Specifically, I follow selected cohorts of women over time and analyze their childbearing decisions for the extensive and intensive margins of fertility. This enables me to show that the labor demand shock impacted the timing of childbearing, but also had a persistent impact on individual-level fertility in the long term.

Finally, this paper is related to studies analyzing plant closures, which arrive at different conclusions. For Finland, Huttunen and Kellokumpu (2016) show that female job loss decreases fertility. Del Bono et al. (2015) also find negative effects of job loss on the fertility of female white-collar workers in Austria, which they attribute to career disruptions.[7] This demonstrates the importance of the type of demand shock investigated. Mass layoffs have severe implications at individual and regional levels, but a significant fraction of displaced workers move into new jobs relatively quickly (Gathmann et al., 2017). Thus, after a plant closure, women seem to prioritize their reentry into employment before they have children. By contrast, a structural demand shock affecting entire industries may impact previously

[5] See also Perry (2004) who explores heterogeneity depending on women's qualification. Early contributions in this area are Schultz (1985) and Heckman and Walker (1990).

[6] Methodologically similar studies analyze shocks to family income or wealth. These studies exploit job displacement of husbands (Lindo, 2010), energy price shocks which increased male wages (Black et al., 2013), or real estate price changes (Lovenheim and Mumford, 2013; Dettling and Kearney, 2014).

[7] Similarly, De la Rica and Iza (2005) show that the high prevalence of fixed-term contracts and a higher threat of job loss were associated with delayed childbearing in Spain.

acquired human capital more permanently, thereby causing the composition effects I analyze in this paper.

The paper proceeds as follows. In the next section, I provide background information on the labor demand shock and its variation across industries. Section 2.3 contains a description of the data and sample. Section 2.4 includes the baseline empirical model. In Section 2.5, I establish that the labor demand shock impacted labor market outcomes. I analyze its effect on fertility in Section 2.6. In Section 2.7, I introduce older cohorts of East German women as a control group to demonstrate the robustness of the results. Section 2.8 assesses the effect on completed fertility. Section 2.9 concludes.

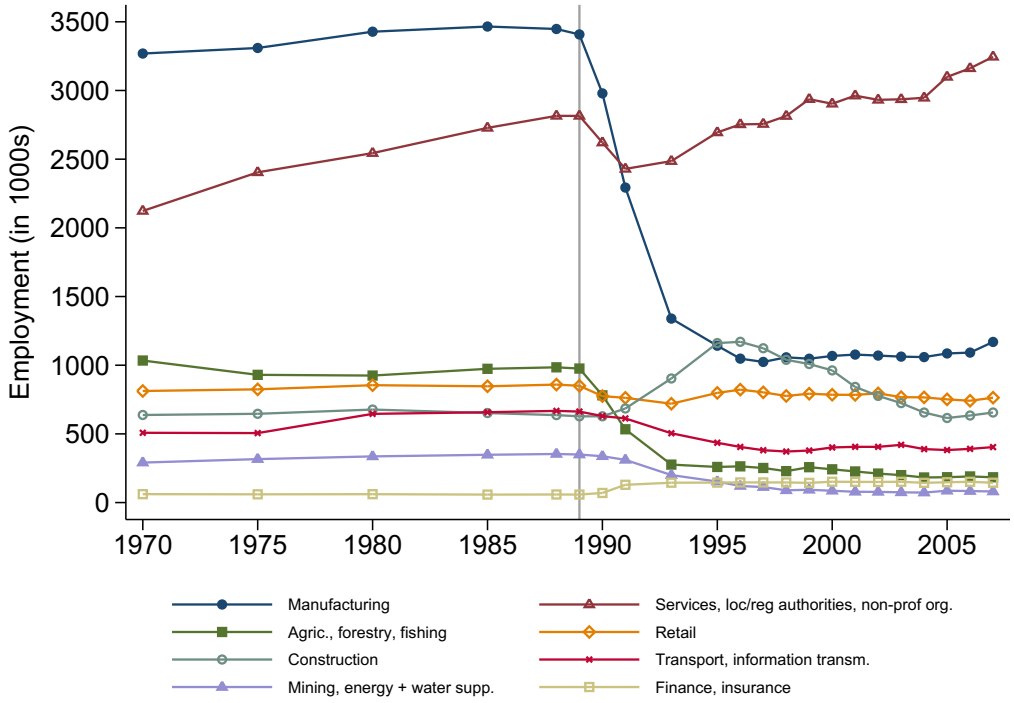
2.2 Background and Empirical Strategy

2.2.1 Selection Into Industries

For my empirical strategy it is important that the self-selection of GDR citizens into jobs was exogenous to the industry-level variation of the negative labor demand shock that I exploit. In this context, two institutional aspects are crucial. First, the reunification and economic integration of Germany were not anticipated. As a result, GDR citizens self-selected into jobs and industries independently of conditions that later prevailed in the market economy. The same argument has been made in the migration literature with regard to pre-determined occupational choices of migrants (Friedberg, 2001; Borjas and Doran, 2012; Prantl and Spitz-Oener, 2014).

Second, under central planning individuals' job choices were heavily constrained (Köhler and Stock, 2004; Baker et al., 2007; Fuchs-Schündeln and Masella, 2016; Prantl and Spitz-Oener, 2014). When Erich Honecker came to power in 1971, access to higher education was severely restricted. Very few students were allowed to obtain the school diploma which qualified for direct university admission. Apart from good performance in school, the demonstration of political loyalty towards the GDR regime and active membership in the "Free German Youth" were necessary prerequisites for being accepted to this school track. Career counseling was meant to influence individuals from an early age onwards to ensure that their occupational choices were made in accordance with available positions. In the sixth school year at the latest, students had to define their desired occupation for the first time. Applications for multiple apprenticeship positions were officially not possible. In sum, self-selection into industries was exogenous to the labor demand shock studied in this paper because German reunification was not anticipated and because job choices in the GDR were

Figure 2.2: Employment in East Germany (in Thousands), By Economic Sector, 1970-2007 (Selected Years)



Source: *1970-1989:* Federal Statistical Office (1994), which recoded data for the universe of all GDR establishments (the so-called *Berufstätigenerhebung*) according to West German classification schemes, and additionally included persons working for the army, Ministry of the Interior, Socialist Unity Party, and the Ministry of State Security as these did not appear in official GDR statistics. *1990:* Bernien et al. (1996, p. 16) based on the last *Berufstätigenerhebung* which also covered the universe of all East German establishments and refers to November 30th, 1990. *1991-2004:* Author's calculations based on Scientific Use Files of the Microcensus (a 0.7 percent sample of the population) for persons aged 15 and older living in East Germany (including East Berlin) at their main residence.

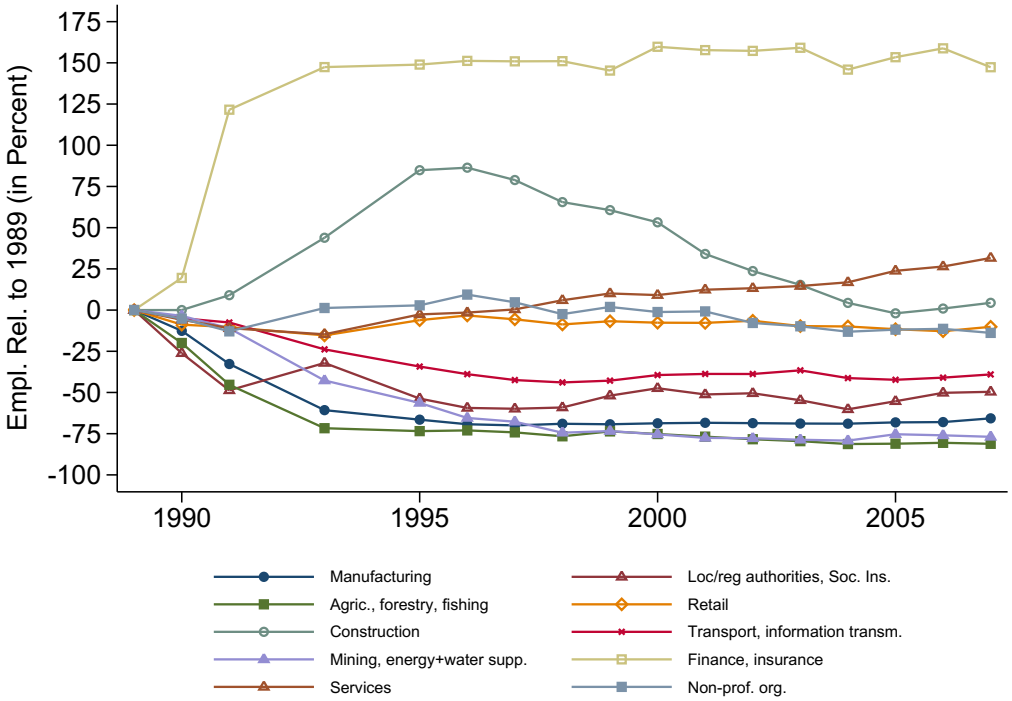
constrained.

2.2.2 Employment Development by Economic Sector

As the market economy was introduced in the formerly communist country, East Germany experienced a sharp reduction in labor demand. This affected individual East German industries differently. I now discuss the differential impact of the labor demand shock across broadly defined economic sectors. At this level of aggregation I could compile reliable and consistently classified employment data spanning several decades. For 1970 to 1990, these data are based on the universe of all East German establishments, and for 1991 to 2007 the data source is the German Microcensus. The time series illustrates that the demand shock is particularly suited to the purposes of my research question.

Figure 2.2 shows absolute East German employment by sector from 1970 to 2007. The

Figure 2.3: Employment in East Germany Relative to 1989 (in Percent), By Economic Sector



Source: As in the previous figure. From 1989 onwards it is possible to split up the large category of services, local and regional authorities, and non-profit organizations.

figure reveals that before 1989 sectoral employment structures were remarkably stable in the GDR. This stability reflects that central planners pursued an extensive growth strategy, which was based on a mere expansion of production. There was no transition to an intensive growth strategy, which would have fostered productivity increases and corresponding adjustments of the sectoral structure. In the 1980s, the political authorities of the GDR failed to reallocate workers across sectors. One reason was that firms engaged in labor hoarding. Also, workers were reluctant to leave their firms as these had an important social function. The resulting sectoral stability was possible only because full employment was guaranteed by the state. This included full employment of women (Grünert, 1996; Ritter, 2007).

There was a structural break due to German reunification, which unexpectedly and permanently changed the East German employment distribution over sectors. This is further illustrated in Figure 2.3, which displays relative employment changes by sector after 1989. Employment losses were especially drastic in agriculture, manufacturing, and "mining, energy and water supply" where employment declined by up to 75 percent until 1993. The second highest relative employment losses were in local and regional authorities, and in

transport and information transmission. Much less pronounced employment losses occurred in retail, not-for-profit organizations, and services. The service sector even grew from 1993 onwards. Finally, the rather small finance and insurance sector grew strongly, and the construction sector experienced a boom which lasted until 1996.[8]

The East German employment decline after the fall of the wall and its variation across sectors were driven by three main phenomena (Lutz and Grünert, 1996). First and most importantly, the former GDR economy had to adjust to the fact that there were clear differences in economic structures between the GDR and market economies such as West Germany. It is crucial for my empirical strategy that, between East and West Germany, there were pronounced differences in the distribution of workers across broad sectors and more detailed industries. Second, employment declined due to migration to West Germany, early retirement schemes, and layoffs of workers with low performance who had been guaranteed jobs in the GDR. Finally, many workers in the so-called "Sector X" lost their jobs. These workers had previously been employed by the army, the Ministry of the Interior, the Ministry of State Security, and the Socialist Unity Party. Personnel replacements also impacted academic disciplines related to the economic and social system of the GDR.

2.2.3 Industry-Level Variation of the Labor Demand Shock

In the analysis, I will rely on variation of the labor demand shock at a level that is more detailed than broad sectors and will distinguish between 48 different industries. To give examples within the manufacturing sector, employment in the textiles and wearing apparel industries declined by more than 80 percent between 1989 and 1993, compared with a 57 percent employment decline in the food production industry, and a 41 percent decline in the chemical industry. Within services, the category "other services (consulting and related activities)" had declined by 19 percent by 1993, whereas the category "accommodation, homes, laundry, cleaning, waste collection" had increased by 40 percent.[9]

These relative changes in employment reflect both demand-side and supply-side adjustments. A credible identification strategy, however, should circumvent supply-side adjustments, as they are potentially endogenous. In particular, supply-side adjustments could be related to childbearing decisions. Thus, I derive an exogenous measure for the varying intensity of the labor demand shock where I exploit that the employment distribution over

[8] To compare the East German case to a market economy, in Appendix Figure A.1 I plot the development of West German employment by economic sector. In West Germany, changes to the sectoral structure started earlier and were more gradual. There was no structural break after 1989.

[9] 1993 was chosen as the reference year, because the major employment changes occurred up until 1993; also after 1993 there was a change in the classification scheme of these more detailed industries.

industries differed strongly between East and West Germany in 1989. Specifically, I define the following measure of relative excess supply (RES):

$$RES_{j,89} = \frac{(Empl\_East_{j,89}/Empl\_East_{89}) - (Empl\_West_{j,89}/Empl\_West_{89})}{Empl\_East_{j,89}/Empl\_East_{89}}, \quad (2.1)$$

where $Empl\_East_{j,89}$ denotes the number of East German workers employed in an industry j in 1989, and $Empl\_East_{89}$ stands for total East German employment in 1989. $Empl\_West_{j,89}$ and $Empl\_West_{89}$ are defined analogously for West Germany. The numerator accounts for percentage point differences in East and West German industry shares in 1989. The larger the numerator is, the greater is the excess supply of East German workers in an industry j relative to the West German market economy benchmark. Accordingly, one can expect East German employment in j to decline. The denominator of the RES measure relates this percentage point difference to the relative size of an East German industry, since a given percentage point difference should matter the more, the smaller is an East German industry. Importantly for identification, the RES measure is entirely based on differences in industrial employment structures that emerged because of divergent economic developments in East and West Germany during the separation of the country.[10] Thus, the RES variable is exogenous to supply side adjustments after the fall of the wall, such as potentially selective fertility decisions, migration to West Germany, or other movements out of the East German labor force.

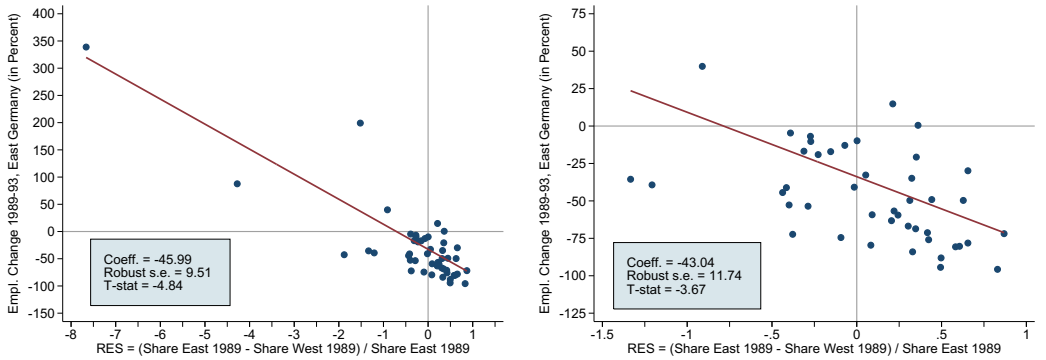
In panel (A) of Figure 2.4, I regress relative employment changes by industry on the RES measure. The figure illustrates the strength of the RES measure as a predictor for the relative employment decline of East German industries after 1989. Relative employment changes are captured by the percentage change in employment of an East German industry between 1989 and 1993. The figure confirms that relative employment changes of East German industries are indeed negatively correlated with 1989 employment differences in East and West German employment structures, as measured by the RES variable.[11] As shown in Panel (B) of the same figure, where the four most extreme RES values are excluded, this negative relationship is not driven by outliers.[12]

[10] To emphasize that the East German employment changes were indeed the result of reunification, in a placebo test I regress West German employment changes on the RES measure. These two variables are not systematically correlated (Appendix Figure A.2).

[11] Due to a lack of female GDR-employment statistics, the RES-measure includes both genders. A measure based on women alone might be more precise, implying that I present lower bound effects. In particular, East German women were more likely than men to lose their jobs after reunification (Hunt, 2002). In Appendix Figure A.3, I confirm that, across industries, job losses were more severe for women in 1993. There is some, albeit very imprecisely estimated, evidence that female relative to male job loss was stronger in industries developing more favorably (with low RES-values).

[12] 'Social insurance agencies' are excluded from the analysis but discussed in the Appendix. In 1989, only 0.07 percent of East German workers were employed in this industry. Most workers initially employed in this

Figure 2.4: Correlation between the Relative East German Employment Change 1989 to 1993 (in Percent) and Relative Excess Supply in 1989, By Industry



A. All industries (N=48) B. Excluding the four most extreme RES-values

Source: For East Germany in 1989 and 1993, the data sources are the same as in Figures 2.2 and 2.3. For West Germany in 1989, the data source is the Microcensus. See Appendix Table A.4 for a list of the industries. The regression models are weighted using 1989 East German employment shares of industries as analytical weights. The y-axis displays change in percent, i.e., $100 * \frac{Empl\_East_{j,93} - Empl\_East_{j,89}}{Empl\_East_{j,89}}$. In panel (B), the four most extreme RES-values are excluded (these are the finance and insurance industries, "printing & reproduction," and "finishing trade") to emphasize that the relationship in panel (A) is not driven by outliers.

The economic rationale why the RES measure predicts East German employment changes after 1989 is twofold. First, market forces led to a convergence of the distribution of East German employees to the West German standard. After all, West German industry structures had evolved such that the West German economy was relatively successful internationally. Second, as part of the massive privatization of East German firms by the "Trust Agency," decisions were made on a case-by-case basis, while industrial policy concerns such as regional spill-over effects played a subordinate role. In this context, East German firms were frequently taken over by and integrated into West German firms belonging to the same industry. The East German firms were, for example, then established as suppliers of intermediary input goods (Wahse, 2003; Federal Institute for Special Tasks Arising From Unification, 2003). Essential for the identification strategy is the fact that East Germans did not anticipate any of these developments. At the point in time when former GDR citizens sorted into industries, industrial employment structures were stable, employment was guaranteed by the state, and sorting into industries was constrained as a result of limited job choice under central planning.

industry were replaced (Bernien et al., 1996), and the drastic expansion of this industry had no positive impact on their later labor market success.

2.3 Data and Sample

2.3.1 Main Data: BASiD

The "Biographical Data of Social Security Insurance Agencies in Germany 1951-2009" (BASiD) combine data from the German Statutory Pension Insurance Scheme (*RV*), the Federal Employment Agency (*BA*) and the Institute for Employment Research (*IAB*).

The basis of BASiD is the Sample of Insured Persons and their Insurance Accounts 2007 (*Versichertenkontenstichprobe, VSKT*) from the RV, which is merged with data from the BA and the IAB. The VSKT 2007 is a 1 percent sample of insured persons aged 15 to 67 at December 31, 2007 who are still alive and have an active pension insurance account. This refers to persons who are covered by the pension insurance scheme but are not currently receiving pensions.[13] Insured persons contribute to their pension entitlements by means of employment, child care or elderly care, by receiving health insurance in case of long-term illness, or by receiving social benefits such as unemployment insurance (for more details about the data see Hochfellner et al. (2011, 2012)).

The BASiD data have a rich panel structure. Up until 2007, they provide retrospective information on all spells and events which are relevant to the pension insurance, the unemployment insurance, or both. For the purposes of my study, the BASiD data have three major advantages. First, I can identify former GDR citizens in the data even if they moved to West Germany after the fall of the wall. As large proportions of young East German women migrated to West Germany during the 1990s (Hunt, 2006; Fuchs-Schündeln and Schündeln, 2009), this is an important feature. It enables me to include East German women who migrated to West Germany in my analysis. Second, the data provide accurate information on the month of birth of a woman's children, because childbearing entails contributions to pension entitlements. This is also true for births before 1989. Finally, sample sizes of BASiD are considerably larger than in alternative German panel data sources, which allows me to analyze the extensive and the intensive margin of fertility over a relatively long time period of 17 years.

The BASiD data are well suited to study how women's labor market situations impact childbearing decisions. By contrast, information on births is unfortunately not available for men. A potential concern in this context is assortative mating. However, I later add imputed control variables for the presence and labor market prospects of spouses and show that the results are not driven by assortative mating. Moreover, it is a priori reasonable to

[13] German pension data have high coverage rates (Richter and Himmelreicher, 2008), but, as is typically the case for German administrative data, the self-employed and civil servants are not included.

expect that the labor market situation of East German women had a significant impact on their childbearing decisions. East German women and mothers have traditionally had a high labor force attachment (Rosenfeld et al., 2004). Among East German mothers with minor children in 1996, only 7.7 percent of mothers with partners and 2.2 percent of single mothers reported transfers from current partners, former partners or other relatives as their main income source (the corresponding figures for West Germany are 50.2 and 9.8 percent). While West German mothers relied much more often on their partners' incomes, for East German mothers the most important income sources were their own wages and salaries, followed by public transfers (Federal Statistical Office, 2010, p. 26). Adler (1997) emphasizes the reluctance of East German women to economically depend on their partners. Providing qualitative evidence, she even argues that economic independence from men would be a prerequisite for East German women to have children.

2.3.2 Sample Selection

Identification of East Germans

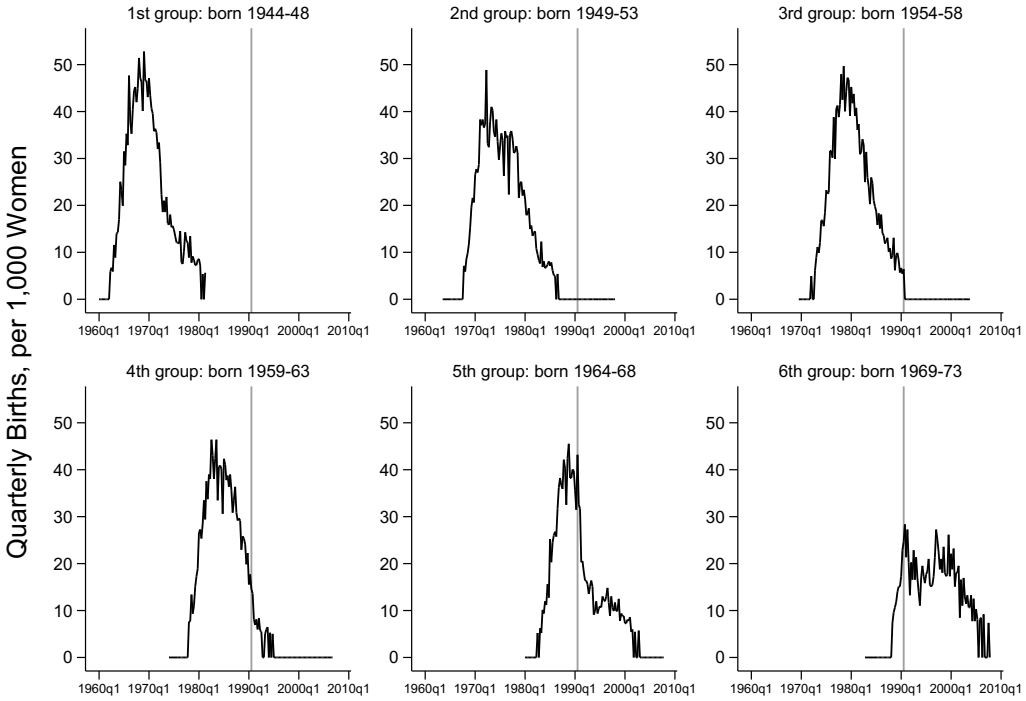
The selection of the sample of East German women requires three steps. In the first step, I identify East Germans by exploiting the fact that contributory periods in East and West Germany yield different pension entitlements. Specifically, the sample includes women who prior to 1989 had at least one spell related to work or training in the dual system of apprenticeship in the GDR, but had no such spells in West Germany prior to 1989.[14] These criteria ensure that the selected women were integrated in the East German labor market and that the labor demand shock studied in this paper was relevant to them. Note that women who migrated to West Germany after the fall of the wall are kept in the sample to rule out selective attrition resulting from migration to the West.

Cohort Choice

The second selection criterion concerns the cohorts included in the analysis. Women in the former GDR had their children at young ages, particularly in comparison with West German women (Huinink and Wagner, 1995; Goldstein and Kreyenfeld, 2011). This is confirmed in Figure 2.6, where I display the number of quarterly births per 1,000 East German women born between 1944 and 1973, based on the BASiD data and for six different cohort groups. The fertility of women who were older than 31 in 1990 (or born before 1959) was largely

[14] I am grateful to Dana Müller whose Stata-routine I use to distinguish between East and West German spells. See also Grunow and Müller (2012).

Figure 2.6: Quarterly Number of Births per 1,000 East German Women, By Six Cohort Groups



Source: BASiD; based on women who, prior to November 1989, worked or did apprenticeship training in the GDR but not in West Germany, and including women who migrated to West Germany after November 1989. $N = 22,572$. Due to data protection, less than 20 absolute births per quarter had to be censored and are displayed here as zero births. The vertical line stands for the third quarter of 1990. See Section 2.3.1 for details on the data and Section 2.3.2 for details on the identification of East Germans.

completed even before the wall fell (groups 1-3 in Figure 2.6). By contrast, childbearing decisions of women born between 1959 and 1973 were impacted by the fall of the wall (groups 4-6). Therefore, I include groups 4 to 6, who were aged 17 to 31 at the end of 1990, in the main sample.

Final Selection Step

The final step in the selection of the sample arises from a peculiarity of the BASiD data. In this step, the sample is additionally restricted to women who on January 1st, 1991, worked in East Germany and have non-missing industry information. This is the first point in time for which industry information is known for a subsample of East Germans. The final sample consists of 4,234 women.

To explain the final step, after reunification the East German labor administration was integrated into the West German administration as part of a complex process (Schmid and

Oschmiansky, 2007). For some firms industry information was reported already in 1991, whereas it is available for all East German firms from 1992 onwards. It is, however, crucial to infer industry information for the earliest point in time possible, since industries are only observed for employed persons. Unemployment rapidly increased from 9.5 percent among East German women in mid-January of 1991 to 20.5 percent a year later (Federal Employment Agency, 2015) and a significant fraction of workers likely changed industries after they became unemployed. Figure 2.3 above shows that the most pronounced employment losses took place after 1990 (in this figure, the information for 1990 refers to the end of November). Using industry information from January 1st, 1991, thus seems to be a good approximation of industries prior to the fall of the wall. As far as the fertility analysis is concerned, it is also reasonable to exclude the year 1990. Fertility can respond to economic conditions only with a time lag of nine months, and during 1990 births were largely independent of the fall of the wall in November of 1989.

It needs to be emphasized that industry information for January 1st, 1991, is missing for a large fraction of women who work on this day (there are 4,234 employed women with non-missing and 4,783 employed women with missing industry information). It is important that the missing information on industries is driven by firms not yet integrated into the social security system rather than by workers not reporting their industry. Workers sorted into firms prior to reunification and independently of later labor demand conditions. Nevertheless, the fact that information on industry is missing for a large fraction of firms requires further investigation.

To begin with, the representativeness of the sample distribution over industries is a concern. For example, at the sectoral level, the sample share of women working in mining is suspiciously large and the sample share working in services appears to be too small. To correct for these discrepancies between the sample distribution and the population distribution, I use the Microcensus of 1991. In the Microcensus, I identify women born between 1959 and 1973 who live in East Germany and compile their distribution over industries.[15] For each woman from an industry j, I then calculate the following simple post-stratification weight:

$$w_j = \frac{share\_Microcensus_j}{share\_sample_j}, \tag{2.2}$$

where $share\_Microcensus_j$ is an estimate of the population share of this industry. Throughout the paper, I apply w_j as probability weights.[16]

[15] I use the Scientific Use File, which is a 0.7 percent representative sample of the population. The Microcensus was conducted in April, when unemployment was already high. Therefore the distribution is based on the current industry of employed women and the last industry of non-employed women.

[16] Weighted and unweighted results are consistent in a qualitative sense, but effects in unweighted estimations

In addition, it is possible that the firms included in the analysis are not a random sample. However, in a robustness test I control for a set of firm-level characteristics and show that these characteristics do not confound the impact of the labor demand shock on fertility.

Finally, there might be selective sample attrition because some women have already lost their jobs by January 1st, 1991. 8 percent of women are unemployed on this day and I discuss this specific group in Appendix A.2. It turns out that the initially unemployed women are indeed a selective subgroup, since they include a high share of mothers with young children who were born immediately before the fall of the wall. At the same time, this corresponds to the intuition behind the findings for the main analysis, as it indicates that in these times of elevated uncertainty, childbearing was related to the risk of job loss. In Appendix A.2, I also provide fertility statistics for employed women with missing and non-missing industry information for January 1st, 1991. Overall, fertility statistics for these two groups compare well. Relative to these two groups, it is again the case that women who had already lost their jobs by the beginning of 1991 had relatively many children on average.

2.4 Baseline Estimation

For woman i from industry $j = 1,...,48$ and years t, the following panel regression is estimated:

$$Y_{itj} = \beta_0 + \beta_1 RES_{j,89} + X'_{it}\beta_2 + X'_i\beta_3 + \gamma_t + \varepsilon_{itj}. \tag{2.3}$$

The model is estimated for three separate time periods containing years 1991 to 1994, 1995 to 1999, and 2000 to 2007, respectively. $RES_{j,89}$ is the measure for the intensity of the unexpected and exogenous labor demand shock as derived in Section 2.2.3. γ_t are time fixed-effects. Since the treatment is time invariant within industries, throughout the paper robust standard errors are clustered at the industry level (the number of clusters is 48).

I first establish that the RES demand shock impacted different labor market outcomes, which are discussed in the next section. I then analyze the impact on fertility, where Y_{itj} is a dummy variable equal to one if a woman gave birth in a given year.

To assess how the impact of the demand shock evolves over time, I focus on three distinct time periods. The first period of years 1991 to 1994 refers to the short term and includes the years during which aggregate East German fertility plummeted. The second period captures medium-term effects. It includes years 1995 to 1999 during which aggregate East German fertility increased again (for these aggregate fertility trends recall Figure 2.1 above). Finally, the third period is defined as years 2000 to 2007 and refers to the long term. The

tend to be somewhat smaller and their statistical significance tends to be weaker.

Table 2.1: Summary Statistics for the RES Measure and the Control Variables

Panel A: RES Measure			
Mean	-.204	P90, P10 interval	[.583, -.911]
Standard deviation	(.993)	P75, P25 interval	[.324, -.391]
Panel B: Control variables			
Age (here: in 1991)	25.68	2 children bf. 91	.264
S.d. of age	(4.11)	3 children bf. 91	.034
Low Qualification, 91	.255	Privileges in GDR	.057
Med. Qualification, 91	.706	Appr. Training, 91	.062
High Qualification, 91 (reference)	.039	Large city, 91	.099
0 children bf. 91 (reference)	.386	Very large city, 91	.141
1 child bf. 91	.316		
Sample size; i.e., individuals observed each year (1991-2007)			4,234

Notes: Panel A refers to the industry a woman worked in on January 1st, 1991. In Panel B all control variables are defined as dummy variables (except for age). The sample is balanced with $N = 4,234$ individuals.

main parameter of interest, β_1, measures the average annual impact of the unexpected and exogenous labor demand shock during the respective time periods. β_1 thus summarizes annual effects in a way that is straightforward to interpret. I later augment the baseline specification to investigate how the effects accumulate over time.

Across specifications, time variant control variables are age and age squared. The other control variables are time constant. These include two qualification dummy variables referring to women who in 1991 had no formal qualification and to women who in 1991 had completed apprenticeship training, respectively. Women who in 1991 had graduated from university are the reference category. It is important to control for qualification, since the qualification level of the employed in an industry might have influenced both the RES-variable and the fertility rate. Further control variables are dummy variables for the number of children a woman had prior to 1991 (one child, two children and three or more children), and two dummy variables for whether a woman worked in a large city or very large city at the beginning of the 1990s, because fertility patterns as well as industry structures might differ in rural versus urban areas. Another dummy variable for persons who in the GDR were entitled to privileged pensions serves as a proxy for closeness to the regime. This variable addresses the concern that for some workers labor market trajectories after reunification were shaped by their prior regime closeness rather than their initial industry. A final dummy variable captures whether a woman was still in apprenticeship training in 1991. A more detailed description of the definition and in some cases of the imputation of variables is provided in Appendix Table A.3. Summary statistics for the RES demand shock measure

and for the control variables are shown in Table 2.1. Summary statistics of the dependent variables are included in the respective tables below.

2.5 Analysis of Labor Market Outcomes

I now establish that the RES demand shock generated exogenous variation in individuals' labor market outcomes (Table 2.2).[17] The first outcome variable is the incidence of unemployment, which is captured by a dummy variable equal to one if a women experienced an unemployment spell in a given year. The second outcome variable is the duration of unemployment expressed in months per year, which is set to zero for women without any unemployment spell. Third, industry changes are defined as a dummy variable equal to one if a woman started to work in a new industry in a given year. Finally, migration to West Germany is accounted for by a dummy variable equal to one in the year migration took place.

To facilitate the interpretation of results, I compare estimated effects for women who initially worked in industries subject to a severe labor demand shock with estimated effects for women who initially worked in industries which were less severely hit. Specifically, I compare women at the 90th percentile of the RES measure (which is 0.58 and stands for a severe labor demand shock) with women at the 10th percentile (which is -0.91 and implies the industry was not negatively impacted by the shock). In terms of actual industries, the 90th percentile coincides with textile manufacturing, whereas the 10th percentile corresponds to lower-skilled services including cleaning and laundry workers. Throughout the paper, tables include rows labeled "P90 vs P10." In these rows, the difference in estimated effects between the 90th and the 10th percentile is reported.

As shown in Table 2.2, the impact of the RES demand shock on unemployment is positive, significant, and it persists over time. In the short term of years 1991 to 1994, the implied differential increase in the incidence of unemployment is 7.0 percentage points on average per year when comparing a worker at the 90th percentile with a worker at the 10th percentile (Table 2.2, panel a, column 1). The average implied increase in unemployment duration per year is 0.48 months (panel b, column 1). When additional controls are added, the effect on unemployment is only slightly smaller (panels a and b, column 2). Over time, the effect decreases, but it remains positive even in the long term (panels a and b, columns 5 and 6). Besides unemployment, the RES demand shock also impacted mobility across industries. For the short-term period, a worker at the 90th percentile is estimated to be around 3.3 percentage points more likely to change industries in a given year than a worker at the 10th percentile

[17] Note that I now refer to the "RES demand shock" or the "RES measure" to distinguish the industry-level variation from the aggregate demand shock.

Table 2.2: Relative Excess Supply (RES) and Various Labor Market Outcomes, OLS Estimates (Panel Regressions)

	(1)	(2)	(3)	(4)	(5)	(6)
	1991-94		1995-99		2000-07	
(a) Dep. Var.: Unemployment Spell in t (0/1)						
RES	.047***	.040***	.039***	.033***	.021**	.017**
	(.015)	(.013)	(.009)	(.008)	(.008)	(.007)
P90 vs P10	.070	.059	.058	.050	.031	.025
Mean of dep. var.	.364	.364	.299	.299	.213	.213
(b) Dep. Var.: Unemployment Duration in t (months)						
RES	.319***	.264***	.286***	.240***	.174**	.140**
	(.101)	(.087)	(.057)	(.047)	(.072)	(.059)
P90 vs P10	.475	.394	.426	.358	.260	.208
Mean of dep. var.	2.431	2.431	2.101	2.101	1.505	1.505
(c) Dep. Var.: Industry Change in t (0/1)						
RES	.022***	.021***	.015***	.015***	.008***	.007***
	(.008)	(.008)	(.005)	(.005)	(.002)	(.002)
P90 vs P10	.033	.032	.023	.022	.012	.010
Mean of dep. var.	.182	.182	.120	.120	.059	.059
(d) Dep. Var.: Migration West in t (0/1)						
RES	-.002	-.002	.000	.000	.002**	.002***
	(.002)	(.002)	(.001)	(.001)	(.001)	(.001)
P90 vs P10	-.003	-.003	.000	.000	.003	.003
Mean of dep. var.	.032	.032	.016	.016	.022	.022
Basic controls only	yes	-	yes	-	yes	-
Main controls	-	yes	-	yes	-	yes
Time FEs	yes	yes	yes	yes	yes	yes
N (each year)	4,234	4,234	4,234	4,234	4,234	4,434

Notes: Each coefficient is from a separate regression. "Basic controls" are age, age squared and two dummy variables for 1991 qualification (no formal qualification and apprenticeship training; university graduates are the omitted category). "Main controls" additionally include dummy variables for the number of children born prior to 1991, for living in a large or very large city in 1991, for GDR regime closeness, and for apprenticeship training in January 1991. Robust standard errors clustered at the industry level are in parentheses; ***, **, * refers to significance at the 1, 5, and 10 percent level, respectively. "P90 vs P.10" columns report the differences in the estimated effects between the 90th and the 10th percentile. The 90th percentile of the RES measure equal 0.58 and stands for a severe labor demand shock; the 10th percentile equals -0.91 and implies that the industry was not negatively impacted by the shock. Hence, the difference between the 90th and 10th percentile equals "coefficient" multiplied by 0.58-(-0.91)=1.49. Results are weighted using post-stratification weights as explained in Section 2.3.2.

(panel c, columns 1 and 2). Again, this effect decreases over time but remains positive.[18] Finally, there is no systematic association between the RES measure and the decision to migrate to West Germany (panel d of Table 2.2).[19] This implies that the results on fertility

[18] Participation in retraining programs could be another adjustment mechanism, but information on such programs is not available before 2000. Wage effects are also neglected, because after reunification East German wages were determined as part of a political process influenced by West German unions. Thus, wages exceeded market equilibria (Krueger and Pischke, 1995).

[19] Note that I did not find a systematic association between RES and the duration of the time spent in West

presented below are not confounded by migration to West Germany.

The impact of the RES demand shock on unemployment and mobility across industry is economically significant. This is particularly true if one keeps in mind that the RES measure exploits only one dimension of the labor demand shock. Moreover, it impacted unemployment and mobility across industries over a relatively long time period.

2.6 Baseline Fertility Analysis: The Composition Effect

2.6.1 Annual Births

Based on the previous section, it follows that for women who initially worked in industries subject to a relatively severe labor demand shock, labor market outcomes were less favorable and stable compared with their counterparts who initially worked in industries less strongly affected. I now assess how the demand shock impacted fertility and regress annual births on the RES demand shock measure. A distinction is again made between the short term, medium term, and long term. The results are reported in Table 2.3.

Throughout the 1990s, the RES demand shock affected the composition of mothers, which I refer to as the "composition effect." In particular, during the short-term period of years 1991 to 1994, East German women more severely impacted by the labor demand shock had relatively more children than their counterparts who were less severely impacted. Again, I compare the two extremes of women at the 90th percentile of the RES measure with women at the 10th percentile of the RES measure. Between those extremes, in the short-term period, the annual likelihood of having a child was 0.53 percentage points higher for women more severely impacted by the demand shock (Table 2.3, panel a, column 1). This effect is robust when further control variables are added (panel a, column 2). In the medium term of years 1995 to 1999, the RES demand shock had an even larger positive effect: During these years, women more severely impacted by the demand shock (90th percentile) were 0.76 percentage points more likely to have a child in a given year than women employed in industries not negatively impacted by the demand shock (10th percentile; see panel a, columns 3 and 4). Thus, throughout the 1990s, the RES labor demand shock affected the composition of mothers. This composition effect is economically significant. To put it into perspective, the average annual birth rate during these years was 4 percent. Finally, in the long term of years 2000 to 2007, point estimates suggest a negative, though statistically insignificant, impact

Germany, either; as measured in terms of months spent in West Germany in a given year.

Table 2.3: RES and Annual Births, OLS Estimates (Panel Regressions)

	(1)	(2)	(3)	(4)	(5)	(6)	N (each year)
	1991-94		1995-99		2000-07		
(a) Dep. Var: Birth in t (0/1)							
RES	.0036***	.0037***	.0051***	.0051***	-.0010	-.0009	4,234
	(.0009)	(.0012)	(.0007)	(.0007)	(.0009)	(.0011)	
P90 vs P10	.0053	.0055	.0076	.0076	-.0015	-.0013	
Mean, dep. var.	.040	.040	.041	.041	.019	.019	
(b) Dep. Var: First Birth in t (0/1)							
RES	.0053**	.0053*	.0046***	.0042***	-.0035***	-.0033***	1,597
	(.0022)	(.0029)	(.0008)	(.0009)	(.0007)	(.0008)	
P90 vs P10	.0079	.0079	.0068	.0063	-.0052	-.0049	
Mean, dep. var.	.058	.058	.051	.051	.019	.019	
(c) Dep. Var: Higher-Order Birth in t (0/1)							
RES	.0014*	.0016**	.0034***	.0034***	.0005	.0005	4,234
	(.0008)	(.0007)	(.0008)	(.0008)	(.0008)	(.0009)	
P90 vs P10	.0021	.0023	.0050	.0051	.0007	.0007	
Mean, dep. var.	.018	.018	.021	.021	.012	.012	
Basic contr. only	yes	-	yes	-	yes	-	
Main controls	-	yes	-	yes	-	yes	
Time FEs	yes	yes	yes	yes	yes	yes	

Notes: Explanations are analogous to Table 2.2. The dependent variables are equal to one whenever a woman gave birth in a given year and equal to zero otherwise. In Panel a, this refers to all births, in Panel b to first births, and in Panel c to higher-order births. Results on first births are for women who on January 1st, 1991 were still childless.

of the RES demand shock on annual births: According to point estimates, women more severely impacted by the demand shock were on average 0.13 percentage points less likely to have a child in a given year (panel a, column 6).

To assess whether these results differ by birth order, I separately regress first births and higher-order births on the RES measure (Table 2.3, panels b and c). With regard to first births, the RES demand shock had a pronounced positive effect throughout the 1990s (panel b, columns 1-4), but in the long-term period, this effect turns negative (panel b, columns 5-6). Thus, the RES demand shock appears to have impacted the timing of first births, and some of the effects that are found for the 1990s are later compensated for.

As far as higher-order births are concerned, there is a positive impact of the RES demand shock throughout the short and medium terms (panel c, columns 1-4). In the long term, point estimates are positive but statistically insignificant (panel c, columns 5-6), which indicates that shifting across time mattered less for higher-order births. This suggests a persistent impact of the RES demand shock on higher-order births.

Table 2.4: RES and Total Number of Births/End of Period Childlessness, OLS Estimates (Cross-Sectional Regressions)

	(1) 1994	(2) 1999	(3) 2007
(a) Dep. Var: End of Period Childlessness (0/1)			
RES	-.0215*	-.0422***	-.0162
	(.0119)	(.0110)	(.0112)
P90 vs P10	-.0320	-.0629	-.0241
Mean of dep. var.	.766	.509	.355
N	1,597	1,597	1,597
(b) Dep. Var: Total Number of Births (#)			
RES	.0146***	.0406***	.0334***
	(.0045)	(.0044)	(.0100)
P90 vs P10	.0217	.0605	.0497
Mean of dep. var.	.163	.367	.522
N	4,234	4,234	4,234
Main controls	yes	yes	yes

Notes: Each regression is based on a single cross-section referring only to the year of 1994, or 1999, or 2007, respectively. Regarding "total number of births," the dependent variable is the number of children born 1991-94 (column 1), born 1991-99 (column 2), and born 1991-2007 (column 3). "End of period childlessness" refers to a dummy variable equal to 1 if a woman is still childless at the end of a given year; here the sample is restricted to women who were childless at the beginning of 1991. The estimations contain all main control variables, which are defined as in Table 2.2. Robust standard errors are again clustered at the industry level.

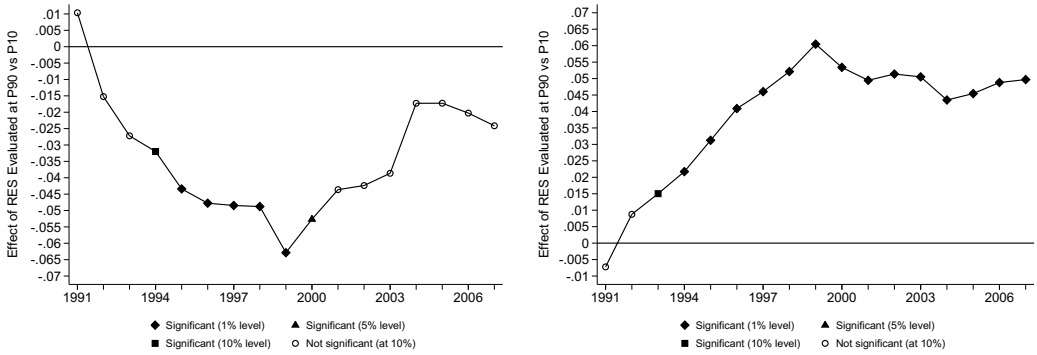
2.6.2 Persistence over Time

Since the industry-level demand shock caused more-severely-affected women to have relatively more children throughout the 1990s, a subsequent question is whether this pattern accumulates to persistent differences in fertility. To investigate this further, I adjust the empirical model to only include one cross-section at a time, namely the cross-sections of 1994, 1999, or 2007. The choice of these years is motivated as before, when I defined 1994 as the end of the short term, 1999 as the end of the medium term, and 2007 as the end of the long term.[20] For woman i, industry $j = 1,...,48$, and year t equal to 1994, 1999, or 2007, the cross-sectional regression model has the following form:

$$Y_{itj} = \beta_0 + \beta_1 RES_{j,89} + X'_{it}\beta_2 + X'_i\beta_3 + \varepsilon_{itj}. \tag{2.4}$$

[20]The year of 2007 is exogenously determined by the right censoring of the data. While the choice of 1994 and 1999 as cutoffs is somewhat arbitrary, the aim is to analyze how the effects accumulate over time and the chosen approach is suitable to reveal this.

Figure 2.7: RES and Total Number of Births/End of Period Childlessness, Graphical Illustration of OLS Estimates (Cross-Sectional Regressions), Years 1991 to 2007



A. Dep. Var: End of year childlessness (0/1); initially childless women only

B. Dep. Var: Total Number of Births (#); all women

Notes: Analogue to Table 2.4. This time, the underlying model (equation 2.4) is estimated separately for all seventeen years. Control variables, standard errors, and "P90 vs P10"-effects are defined as in Table 2.4. "End of year childlessness" refers to a dummy variable equal to one whenever an initially childless woman is still childless at the end of a given year (Panel A). Regarding "total number of births," the dependent variable is the number of children born between the beginning of 1991 and the end of a respective year (Panel B).

To analyze the extensive margin of fertility, the sample is restricted to initially childless women and $Y_{it\,j}$ is a dummy variable equal to one whenever a woman is still childless at the end of a given year (panel (a) of Table 2.4). As one would expect based on the previous results, over time the RES demand shock decreases the likelihood that a woman is still childless. At the end of 1999, a woman at the 90th percentile of the RES measure is around 6 percentage points less likely to still be childless than a woman at the 10th percentile (panel a, column 2). However, up until the end of 2007, the difference in childlessness becomes smaller and is no longer statistically significant (panel a, column 3). This implies that with regard to the decision of initially childless women to become mothers, the demand shock mostly impacted the timing of births. Those women more severely impacted by the demand shock had their first children earlier after they experienced the shock; whereas those less severely impacted postponed childbearing more and became mothers at a later point in time.

By contrast, there is a persistent positive impact on the total number of children born even in the long term. This can be seen in panel (b) of Table 2.4, which refers to the intensive margin of fertility. The sample now includes all women and the outcome variable $Y_{it\,j}$ is the total number of children a woman had between the beginning of 1991 and the end of 1994, 1999, and 2007, respectively. By the end of 2007, there still is a persistent effect of the demand shock on the total number of children born. Between years 1991 to 2007, women more severely impacted by the demand shock have 0.050 children more on average (panel

b, column 3). This effect is economically meaningful, since the average number of births between 1991 and 2007 is 0.522.

Finally, Figure 2.7 complements the analysis, by illustrating estimates of equation 2.4 for all of the available years. Panel (A) shows how the demand shock impacted the timing of first births: While the difference in childlessness at first increases significantly between women more severely impacted by the demand shock and their less-severely-impacted counterparts, from 1999 onward, the same difference becomes smaller. Panel (B) confirms the long-term persistence of the composition effect for the total number of births over the seventeen-year period. From 1992 through 1999 annual birth rates are relatively higher for women more severely impacted by the shock. This leads to accumulated differences in the total number of children born. While this difference does not increase any further from 2000 onward, it persists until the end of the seventeen-year period in 2007.

2.6.3 Robustness

In the two previous sections, I have established that the RES demand shock altered the composition of mothers. I now investigate the robustness of this composition effect. For the sake of brevity, I provide a summary here and include more detailed explanations and results in Appendix A.5.

First, as far as migration to West Germany is concerned, I find evidence for the composition effect among women who stayed in East Germany and women who migrated to West Germany, but the effect is less pronounced for the latter group. Due to new economic opportunities post-migration, for migrants initial industry affiliations likely became less decisive. Note, however, that throughout the analysis I include migrants in the sample, thereby comprehensively assessing the overall effect.

Second, previous research has shown that low-skilled East German women disproportionately lost their jobs after reunification (Hunt, 2002). A concern in this context is that the skill-level of the employed in an industry influenced the RES-measure and also impacted fertility decisions. While I control for qualification throughout the analysis, I additionally conduct a placebo test, where I regress qualification levels on the RES-measure. Reassuringly, there is no systematic association between these variables.

Third, the availability of child care could have an effect on my results. Post-reunification, public child care became the responsibility of local municipalities and its availability declined in East Germany (p. 310 in Kreyenfeld, 2003; Rosenfeld et al., 2004). However, this is not a plausible cause for the composition effect, since in municipalities more severely impacted by the demand shock the availability of childcare should be relatively lower. This should

have a negative effect on birth rates. Moreover, when including municipality fixed effects, I demonstrate that regional spill-over effects merely play a small role in explaining the composition effect.

Fourth, since my sample only includes women employed in firms who reported to the social security system at the beginning of 1991, a concern is that these firms are a selective sample. My results are very robust to the inclusion of firm-level control variables, which suggests that characteristics of firms do not confound my results.

Fifth, the labor demand shock experienced by women could in systematic ways be correlated with their spouses' employment prospects. Because the East German economy was characterized by large firms, assortative mating may have played a role. Yet microeconomic theory and recent empirical evidence suggest that negative shocks to male employment prospects operate through the income effect and therefore depress fertility (Lindo, 2010; Black et al., 2013; Schaller, 2016; Autor et al., 2018). Given that these insights apply to the East German context, the presence of assortative mating should then downward bias the estimated effect of the RES demand shock on the composition of mothers. I present tentative evidence that this is indeed the case, by using imputed control variables for employment prospects of spouses. While I can test these relationships only imperfectly, at the very least my findings cannot be reconciled with the view that the presence of assortative mating drives the effect the demand shock had on the composition of mothers.

Sixth, I investigate non-linearity and exclude the industries with the strongest employment growth (namely the finance and insurance industries, "printing and reproduction" and "finishing trade"; see Figure 2.4 above). The composition effect remains robust, but becomes smaller in the long term. However, once I account for assortative mating, the effect again becomes persistent in the long term.

Finally, I investigate births which occurred before 1991 and hence before the RES demand shock. I find no evidence for unobservable characteristics that are systematically associated with the RES demand shock and fertility. I address the concern of unobservable characteristics more rigorously in Section 2.7 below, when I evaluate the composition effect against a control group of older cohorts.

2.6.4 Interpretation

Throughout the 1990s, East German women more severely impacted by the labor demand shock had relatively more children than their counterparts who experienced a less severe labor demand shock. In terms of neoclassical theory, the substitution effect dominated the income effect in determining the composition of mothers. An interpretation that rationalizes

this finding is that after reunification there was a high degree of general economic uncertainty in East Germany. Up until 2006, the unemployment rates of East German men and women were at dramatic levels (see Appendix Figure A.8). The prevalence of unemployment was also high among the cohorts of women included in my analysis (see Table 2.1). Those women with relatively better labor market prospects might have been less willing to put their jobs at further risk. As a consequence, they had relatively fewer children.

After reunification, the West German system of unemployment compensation was introduced in East Germany (Schmid and Oschmiansky, 2007). Under this system, unemployed persons were first eligible for unemployment insurance (*Arbeitslosengeld*) for several months, provided that their previous employment episodes were sufficiently long. Unemployed persons, who were not eligible for unemployment insurance or who had exhausted their unemployment insurance entitlements, received means-tested unemployment assistance (*Arbeitslosenhilfe*; for details see Hunt (1995); Fitzenberger and Wilke (2004)).

Until 1994, for persons with children unemployment insurance amounted to 68 percent of past earnings; and unemployment assistance to 58 percent. In 1994, the two replacement ratios were each reduced by one percentage point. For persons without children, the two ratios were somewhat lower.[21] After reunification, almost all unemployed East Germans received unemployment compensation payments and were first eligible for the higher unemployment insurance (Schmid and Oschmiansky, 2007, p. 463). As part of the transition process, East German earnings increased significantly (Krueger and Pischke, 1995), which implies that the basis for the determination of unemployment compensation also increased. Overall, during the time that is relevant for my study, unemployment compensation can be considered as relatively generous. This might be one institutional factor explaining why women with less favorable labor market outcomes had relatively more children.

The fact that the RES demand shock had a positive impact on the composition of mothers is puzzling given that East Germany experienced an unprecedented decline in aggregate fertility. In the analysis below, I resolve this apparent contradiction by demonstrating that the labor demand shock impacted both the level of fertility and the composition of mothers. The composition and level effects need to be distinguished, since they did not operate in the same direction.

[21] 63 percent and 56 percent until 1994; 60 and 53 percent after 1994.

2.7 Extended Fertility Analysis: Composition versus Level Effect

2.7.1 Using Older Cohorts as a Control Group

As previously discussed, it is still possible that, across industries, women differ in unobservable characteristics correlated with fertility. To account for this possibility, I define the main sample of women born between 1959 to 1973 as treated cohorts and use older East German women born between 1944 and 1958 as a control group. The fertility of this control group was completed before the fall of the Berlin Wall (recall Figure 2.6) and serves as a benchmark. The identifying assumption requires that the potential endogeneity of industries is constant between treated cohorts and their older counterparts. Thus, the approach accounts for systematic unobserved differences among women in highly affected versus less affected industries under the assumption that these differences are the same for the treatment and the control group. The control group is a natural choice, because the socialization of treatment and control group took place in the GDR and both groups selected into industries within the same institutional environment. The comparison to this control group is moreover informative as it clarifies that the labor demand shock impacted fertility through a level and a composition effect.

To explain the approach in more detail, I also define three time periods for the control group: 1976 to 1979 (short term), 1980 to 1984 (medium term), and 1985 to 1992 (long term). During these intervals, women in the control group were of the same ages as the treated cohorts during the time periods included in the baseline analysis (1991 to 1994, 1995 to 1999, and 2000 to 2007). The following difference-in-differences model is then estimated separately for the respective short-, medium-, and long-term periods:

$$Y_{itj} = \beta_0 + \beta_1 RES_{j,89} + \beta_2 treated\_c_i + \beta_3 treated\_c_i * RES_{j,89} + X'_{it}\beta_4 + X'_i\beta_5 + \varepsilon_{itj}. \quad (2.5)$$

For treated cohorts, t stands for 1994, 1999, or 2007; for control cohorts it refers to 1979, 1984, or 1992. $treated\_c_i$ is equal to one if a woman belongs to the treatment group and equal to zero otherwise. Control variables are analogous to the main controls used in the baseline analysis.[22]

j denotes the industry a woman worked in, which for both treatment and control groups

[22]Note that in the following, I no longer include imputed control variables for husbands (as I have done in Section 2.6.3), since these variables are not available for the control group. Thus, I provide lower-bound estimates of the composition effect.

is defined as her industry on January 1st, 1991. As in the previous section, I distinguish between the extensive and the intensive margins of fertility. The first outcome is end of period childlessness, which is a dummy variable equal to one whenever an initially childless woman is still childless at the end of the short-, medium- or long-term period. The second outcome is the total number of children a woman had between the beginning of the short-term period and the end of a given period.

Regarding main coefficients, β_1 controls for unobserved differences across industries that are correlated with fertility. β_2 captures differences in fertility between the treatment and control group when the two groups were of the same ages. This reflects the impact of the aggregate labor demand shock as well as any other factor changing general fertility trends.[23] Finally, β_3 measures the impact of the RES demand shock on the fertility of treated cohorts relative to the control group. Under the identifying assumption stated above, β_3 estimates the impact of the RES demand shock on fertility net of the influence of a presumed endogeneity of industries. Therefore, β_3 is the crucial parameter when assessing the robustness of the baseline results (specifically the results presented in Table 2.4 of Section 2.6; which I will refer to in the discussion below).

2.7.2 Difference-in-Differences Results

The results of the difference-in-differences analysis are shown in Table 2.5. The table reveals pronounced differences in fertility between treatment and control groups at the same ages. Among initially childless women, women in the treatment group are 25 percentage points more likely to still be childless at the end of the short-term period than their older counterparts (Table 2.5, panel a, column 1, $\hat{\beta}_2$). By the end of the long-term period, the difference in childlessness between treated and control groups has become smaller (panel a, column 3, $\hat{\beta}_2$). This is in line with the finding that part of the East German fertility decline after the fall of the wall was caused by postponement of first births (Conrad et al., 1996; Goldstein and Kreyenfeld, 2011).

In addition, there are strong differences in the total number of births. Compared with the control group, by the end of the long-term period, women in the treatment group had on average 0.279 children less (panel b, column 3, $\hat{\beta}_2$). This clearly shows the relevance of the negative level effect on aggregate fertility. The level effect can be attributed to the aggregate demand shock and to systemic change after German reunification more generally. The results

[23] For example, pro-natalist policies took effect in 1972 and 1976 in East Germany and coincided with higher birth rates among women in the control group (Huinink and Wagner, 1995). This does not invalidate the choice of the control group, as long as these policies - or other factors impacting fertility trends - did not have a differential impact across industries.

Table 2.5: RES and Fertility Relative to the Control Group of Older East German Cohorts, Difference-in-Differences Estimates (based on cross-sections)

	(1) Short term	(2) Med. Term	(3) Long Term	N
(a) Dep. Var: End of Period Childlessness (0/1)				
RES ($\hat{\beta}_1$)	.0040	.0015	.0046	3,190
	(.0222)	(.0131)	(.0048)	
Treated Cohorts ($\hat{\beta}_2$)	.2509***	.2256***	.1215***	
	(.0210)	(.0193)	(.0151)	
RES x Treated Cohorts ($\hat{\beta}_3$)	-.0209	-.0457***	-.0223**	
	(.0214)	(.0122)	(.0087)	
P90 vs P10	-.0311	-.0681	-.0332	
|P90 vs P10| / $\hat{\beta}_2$	.1240	.3019	.2733	
(b) Dep. Var: Total Number of Births (#)				
RES ($\hat{\beta}_1$)	-.0106*	.0007	.0063	8,878
	(.0057)	(.0068)	(.0112)	
Treated Cohorts ($\hat{\beta}_2$)	-.2419***	-.3220***	-.2791***	
	(.0108)	(.0157)	(.0257)	
RES x Treated Cohorts ($\hat{\beta}_3$)	.0227***	.0381***	.0258	
	(.0060)	(.0067)	(.0165)	
P90 vs P10	.0338	.0568	.0385	
P90 vs P10 / $\lvert\hat{\beta}_2\rvert$	.1397	.1764	.1379	
Main Controls	yes	yes	yes	

Notes: "Short term" stands for years 1991-94 for treated cohorts born 1973-59; and for years 1976-79 for the control group born 1944-58. "Medium term" refers to 1995-99 (treatment) and 1980-84 (control); and "long term" to 2000-07 (treatment) and 1985-92 (control). Only the final year of each of the three time periods is included. In panel a, "end of period childlessness" refers to a dummy variable equal to one whenever an initially childless woman is still childless at the end of a given period. In panel b, "Total Number of Births" refers to the number of children born between the beginning of the first period and the end of the final year of the short, medium, and long term period, respectively. Control variables are defined as before, except for qualification now standing for the highest qualification a woman achieved (since 1976 qualification variables are not available for the control group). For the sake of comparability, I use the same post-stratification weights as before.

presented here even seem to indicate that the level effect was so pronounced that it reduced completed fertility among women in the treatment group. Due to the right-censoring of the data in 2007, however, it is beyond the scope of this paper to analyze comprehensively whether this is indeed the case.

Importantly, the results on the impact of the RES demand shock on fertility are robust. Recall that $\hat{\beta}_1$ captures the presumed endogeneity of industries. The parameter is small according to point estimates and almost always statistically insignificant. Thus, a potential

endogeneity of industries is not a concern. The only exception is the impact of the RES measure on the total number of births during the initial period (panel b, column 1, $\hat{\beta}_1$). In this case $\hat{\beta}_1$ is in fact negative. If anything, the positive impact of the RES demand shock on the fertility of the treatment group in the short term has been slightly underestimated in Table 2.4 of Section 2.6.2.

The other parameter of interest reflects the differential impact of the RES demand shock on the fertility of the treatment group relative to the control group, and net of a presumed endogeneity of industries ($\hat{\beta}_3$, see the interaction terms in Table 2.5). $\hat{\beta}_3$ thus captures the composition effect identified in the baseline analysis. According to the composition effect, the intensity of the labor demand shock experienced by women shaped the composition of mothers. Reassuringly, the conclusions drawn are similar to those discussed before. I again find that women more severely impacted by the industry-level demand shock had relatively more children. One difference worth noting is that the negative impact of the RES demand shock on childlessness at the end of the long-term period is now slightly stronger and the statistical significance of this effect has increased compared with the baseline analysis ($\hat{\beta}_3$, panel b, column 3). The opposite is true with regard to the positive impact of the RES demand shock on the total number of children born until the end of the long-term period ($\hat{\beta}_3$, panel a, column 3). The results now suggest that in the long term a woman at the 90th percentile of the demand shock measure had 0.039 births more on average than a women at the 10th percentile of the measure (compared with the previously estimated difference of 0.050 births, see Table 2.4). The statistical significance decreased (the p-value is now 0.125). While the difference-in-differences analysis thus changes some details of a nuanced interpretation of the effects, overall it confirms the persistence of the positive impact of the demand shock on fertility.[24] This shows that the main results are not invalidated by endogeneity.

Finally, a direct comparison of the estimates for $\hat{\beta}_2$ and $\hat{\beta}_3$ in Table 2.5 leads to a more differentiated interpretation. First, the level effect (captured by $\hat{\beta}_2$) and the composition effect (captured by $\hat{\beta}_3$) operate in different directions. The RES demand shock changed the composition of mothers, but against the backdrop of a generally low fertility level. This makes the distinction between the composition effect and the level effect particularly relevant. Second, the level effect is in general relatively large in comparison to the composition effect (as evaluated at the 90th versus 10th percentile). With regard to the total number of births, and depending on the time period analyzed, the magnitude of the composition effect amounts

[24]The interpretation is the same when I right censor the data three years earlier for both treatment and control groups to avoid that the control group is observed after 1989 (not shown). Note, however, that the control group had almost completed their fertility in 1989 (Figure 2.6); and was therefore included until 1992.

Table 2.6: RES and Predetermined Fertility Outcomes Relative to the Control Group of Older East German Cohorts, Test of the Common Trend Assumption (based on cross-sections)

	(1) Prior childlessness (0/1)	(2) Prior total no of births (#)
RES ($\hat{\beta}_1$)	-.0015	.0148
	(.0061)	(.0121)
Treated Cohorts ($\hat{\beta}_2$)	.1085***	-.1896***
	(.0098)	(.0226)
RES x Treated Cohorts ($\hat{\beta}_3$)	.0019	.0045
	(.0106)	(.0246)
P90 vs P10	.0029	.0067
N	8,878	8,878
Main Controls	yes	yes

Notes: The model has the same difference-in-differences structure as in Table 2.5. To test the common trends assumption, the outcome variable in column (1) is a dummy variable equal to 1 whenever a women was still childless before the beginning of the respective short term period (for women in the treatment group this refers to the beginning of 1991 and for women in the control group this refers to the beginning of 1976). In column (2), the outcome is the total number of children born before the respective short term period. Control variables for the number of children born before the short term are obviously dropped; otherwise explanations and control variables are the same as in Table 2.5.

to 14 to 18 percent of the magnitude of the level effect. As far as first births are concerned, and again depending on the time period analyzed, the composition effect amounts to 12 to 30 percent of the level effect (see Table 2.5). While the composition effect was economically significant, it was not strong enough to counterbalance the low aggregate fertility level after the fall of the wall.

2.7.3 Common Trends Assumption

In the last section, β_3 measured the differential impact of the RES demand shock on fertility of the treatment group relative to the control group. The approach requires that estimates for β_3 are driven by the intensity of the negative labor demand shock after 1991. β_3 should not be confounded by any unobservable trend affecting women in the treatment and control groups differently *across industries*. To test and support this assumption, I repeat the previous analysis for predetermined birth outcomes (childlessness at the beginning of the short term period as well as the total number of children born up until this point). This test should yield small and insignificant estimates for β_3. Results reported in Table 2.6 highlight that this is

Table 2.7: RES and Total Number of Births, OLS Estimates (Cross-Sectional Regressions), By Age Groups

	(1) 1994	(2) 1999	(3) 2007	N
(a) Aged 34-38 at the end of 2007				
RES	.0276**	.0797***	.0597*	1,144
	(.0112)	(.0148)	(.0314)	
P90 vs P10	.0433	.1251	.0937	
Mean of dep. var.	.269	.668	1.087	
(b) Aged 39-43 at the end of 2007				
RES	.0017	.0270*	.0227	1,457
	(.0113)	(.0135)	(.0164)	
P90 vs P10	.0025	.0402	.0338	
Mean of dep. var.	.204	.431	.531	
(c) Aged 44-48 at the end of 2007				
RES	.0091*	.0172***	.0149**	1,633
	(.0048)	(.0052)	(.0064)	
P90 vs P10	.0136	.0256	.0222	
Mean of dep. var.	.056	.107	.130	
Main controls	yes	yes	yes	

Notes: Explanations are analogous to Table 2.4. As before, "Total Number of Births" means the number of children born between 1991 and the end of 1994, 1999, or 2007.

indeed the case and support the validity of the difference-in-differences approach.

2.8 Effect Heterogeneity by Age

In the final part of the analysis, I investigate effect heterogeneity by age. Since, in the last section, endogeneity of industries was shown not the be a major concern, I return to the main sample of women born between 1959 and 1973 and distinguish between three different age groups. This analysis is informative as it helps assess to what extent the composition effect impacted completed fertility.

So far, I have found that the composition effect persists even after a relatively long period of 17 years. At the end of this period in 2007, the cohorts I focused on are 34 to 48 years old. While it is possible that some of the identified effects are counterbalanced by later births, childbearing among East German women is not very common after age 34 (recall Figure 2.6 above). It is therefore unlikely that the composition effect completely vanished after the analyzed time period.

Table 2.7 shows that the effect of the RES demand shock on fertility is decreasing in age. Intuitively, this can be rationalized by the fact that the younger women were, the fewer children they tended to have prior to 1991. Therefore, the impact on fertility was stronger for younger women. However, a small persistent effect is found even for the oldest group of women who were aged 44 to 48 in 2007 (Table 2.7, panel c). This last result is of particular interest, because it is impossible that many women belonging to the oldest age group had children after 2007. The result suggests that the RES demand shock had a small impact on their completed fertility.[25]

2.9 Conclusions

In this paper, I analyze how women's labor market situations impact childbearing decisions. I analyze this question in the context of East Germany after the fall of the Berlin Wall. To circumvent the endogeneity of individuals' labor market outcomes, I exploit exogenous variation of the negative labor demand shock which hit East Germany as a result of the introduction of the market economy. The variation stems from differential pressure for restructuring across industries. Industrial restructuring in East Germany was pronounced and entirely unexpected. It led to permanent shifts in employment structures and it generated exogenous variation in individuals' labor market outcomes. Moreover, industrial restructuring occurred independently of previous sorting of East German workers into industries. This makes industrial restructuring in East Germany an interesting test case. It resembles shifts in industrial employment structures in market economies, which were more gradual but also implied a decline in manufacturing and a rise of the services sector.

I find that the negative labor demand shock had an impact on the composition of mothers. Throughout the 1990s, East German women more severely impacted by industrial restructuring had relatively more children than their counterparts who were less severely impacted (composition effect). The comparison with older cohorts of East German women reveals that the composition effect was not strong enough to counterbalance the pronounced negative level effect the aggregate labor demand shock had on the aggregate East German fertility level after the fall of the Berlin Wall (level effect). However, the composition effect was meaningful in an economic sense.

A priori, it is theoretically ambiguous how women's labor market situations impact childbearing decisions. From a neoclassical point of view, there are opposing income and

[25] In Online Appendix Table A.13, I similarly assess heterogeneity by qualification groups. This analysis shows that the composition effect pertains to all qualification groups, but is more pronounced for initially low-skilled and high-skilled women.

substitution effects. The findings presented in this paper point to the dominance of the substitution effect in determining the composition of mothers. This suggests a trade-off between pursuing a career and raising children. After the fall of the wall, economic prospects were generally uncertain in East Germany. In this situation, childbearing likely increased the actual or at least the perceived risk of job loss. As a result, it appears that East German women with more favorable employment conditions were relatively less willing to give up their current labor market situations.

Finally, a general implication of my results is that labor demand shocks impact fertility both through a level effect and a composition effect and that these two effects do not necessarily operate in the same direction. Two questions remain, which concern the generalizability of my findings. First, can we expect the level effect to prevail over the composition effect also in other contexts? This is likely the case, since existing empirical evidence largely finds that fertility is pro-cyclical. Thus, it is generally true that aggregate fertility levels tend to decline during recessions (Sobotka et al., 2011). Second, can we expect to observe the same composition effect also in other contexts? This could be an interesting question for future research, since the answer is a priori not clear. The composition effect might be contingent upon institutional arrangements such as the relatively generous unemployment benefits in Germany prior to the Hartz reforms. However, my paper has demonstrated that, perhaps surprisingly, there are contexts in which level and composition effects of labor demand shocks on fertility do not operate in the same direction. Therefore, composition effects should be carefully investigated also in other contexts, particularly because they entail consequences of economic and social relevance for women and their families.

3 Refugee-Specific Government Aid and Child Refugees' Economic Success Later in Life

Joint with Sandra E. Black, Camille Remigereau, and Alexandra Spitz-Oener

3.1 Introduction

Refugees are a particular class of immigrant who tend to be more highly educated than the average immigrants and who arrive with fewer resources, having fled from political upheaval. Refugees have received a lot of attention in the current political climate, where there is vigorous debate about the large influx of refugees from countries such as Syria and Afghanistan. Many destination countries have programs designed to help these refugees adjust to their new environments, often providing financial support to meet basic needs as well as job-search assistance. By doing so, they acknowledge the challenges of the refugees' experience. At the same time, however, there is growing concern about welfare expenditures on refugees.

Against this background, there is surprisingly little research on how welfare affects the success of refugees.[1] One exception is Andersen et al. (2018), who study a 2002 reform of social assistance in Denmark that cut refugee benefit levels up to 50 percent and changed the modalities of how social assistance is paid. The authors find large and immediate jumps in male employment owing to the reform. However, the change reduced female labor force participation, and the reform made it less likely for children to attend daycare. Another exception is work by LoPalo (2018), who analyzes the effects of cash aid paid to refugees as part of the United States' refugee resettlement program. She finds that cash aid increases wages for the employed but does not alter employment. The highly educated saw the largest wage effect.

[1] By contrast, there is sophisticated knowledge on refugee settlement policies and the consequences of ethnic enclave formation (Edin et al., 2003; Damm, 2009).

The existing research focuses primarily on refugees who arrive as adults, not their children, despite widespread concern that children may face development challenges if their parents start anew with few resources in an unfamiliar country. Similarly, government aid might affect child refugees differently than it does adult refugees. This would be consistent with research in other contexts that shows that age is important when it comes to the success of welfare programs, both theoretically (Mayer and Lopoo, 2008; Del Boca et al., 2016) and empirically (Dahl and Lochner, 2012; Chetty et al., 2016; Chetty and Hendren, 2018). It would also be consistent with results of the previously mentioned study by Andersen et al. (2018), which found that the Danish reform negatively affected the test scores of refugee children.

A divided Germany in the post-WWII era provides us with an opportunity to examine the impact of refugee aid on refugees who arrive as children and young adults. From the end of the war in 1945 until the Berlin Wall was built in 1961, an estimated 3.6 million to 4.5 million East Germans escaped from the communist German Democratic Republic (GDR)[2] and migrated to the Federal Republic of Germany (FRG), or West Germany. Like refugees from other countries, East Germans left behind their belongings and social ties when they escaped from the East. Upon arrival in West Germany, they faced the economic, social, and psychological hardship brought about by flight experiences. But, of course, those newcomers were physically indistinguishable from the West Germans, shared the same language, culture, religious background and - before 1945 - lived in the same country and were involved in the same war, that is, they also shared similar war and other historical experiences.

Despite those similarities, the political climate at the time was such that the GDR refugees were not welcomed with open arms. In contrast, the authorities in West Germany saw the GDR refugees as potential threads to social stability because of common fears that the newcomers were criminals, smugglers, and Communist agents (see Limbach, 2011). Thus, institutions for refugee reception were implemented that involved hosting the refugees first in camps where they underwent severe screening from West German, U.S., British and French authorities (the Allied countries typically involved their intelligence units with the goal to detect communist spies or learn about strategic information the refugees might have). As Limbach (2011, p. 3) states "... there is little difference how the FRG treated East Germans in 1952, and how they came to treat Croatian refugees in 1992 or Afghan refugees in 2010."

At the end of this screening which could take several weeks, GDR refugees were classified as either economic or political GDR refugees. Only the later were considered genuine refugees which - after 1953 - made them eligible for refugee-specific support programs,

[2]We refer to the East German part that in 1949 became the German Democratic Republic as "GDR" throughout the manuscript for ease of exposition. Before 1949 it was the Soviet Occupation Zone.

while both groups could stay in the West. The fact that both groups of refugees could remain in the West is important for our analysis, because it enables us to observe the entire pool of applicants, and not just those refugees who were eligible for aid.

We analyze how refugee-specific government spending impacts the medium-term economic success of refugees who arrived as children (1-14 years old) and as young adults (15-24 years old). Age-at-arrival is an important determinant of integration success, typically for reasons that have to do with language proficiency (Bleakley and Chin, 2010). In our context, given that refugees speak the same language as natives, the institutional connection to the host country is the most important reason for distinguishing the newcomers by age-at-arrival. Children below 15 were subject to compulsory schooling, so they were readily integrated in the West German education system and were then naturally connected to the institutions of their new home country. Those who were 15-24 upon arrival were much less naturally institutionally integrated in West Germany. In principle, they could continue going to school, do an apprenticeship degree, attend university, or immediately start working.[3]

We observe outcomes of the refugee children at least 10 years after arrival. We therefore focus on their medium-term success. It could, for example, be that the impact of refugee-specific government aid is only important for refugee children in the short term and then dissipates with time in the country - particularly in countries where schooling is mandatory and tuition-free, and the social safety net is comprehensive. In contrast with children, young adults might be more vulnerable to an absence of refugee-specific government aid since, again, they are less naturally linked to the educational institutions of the host country. Economic necessity combined with a lack of government assistance may preclude these young adult refugees from pursuing higher education, which might have been beneficial for their long-term well-being.

Our data capture the father's education, the mother's education, the father's industry, and the father's occupational status. These are the most important observable characteristics that the GDR regime tapped to identify potential "class enemies" who needed re-education and other intervention to form "good socialist citizens." Accordingly, the West German and Allied authorities also used these observable characteristics to select genuine political refugees. In addition, the FRG wanted to discourage East Germans from fleeing the GDR in general. One important concern was that if refugee-targeted support programs were implemented for all East Germans fleeing the GDR, this would encourage more to come. As

[3]Note that differences by age-at-arrival in institutional connection to the host country is also important in the context of other refugee groups. The refugee children from Syria, for example, who arrived in Germany below the age of 16 have an immediate institutional connection to their new home country because of mandatory school requirements, whereas this is not the case for older refugees.

a result, only in 1953 were refugee-specific aid programs implemented for the GDR refugees, this came as a surprise, and only genuine political refugees became eligible for aid.

Thus, the general difficult task of identifying the effect of government aid on outcomes owing to self-selection and screening effects is facilitated in our historical setting: we observe the whole pool of applicants, we know the most important variables used by the authorities to select those eligible for aid, and the timing of the refugee-targeted aid program is such that it provides us with exogeneous variation in potential years of exposure to aid, generated by variation in age-at-arrival and year of arrival in West Germany.

Overall, our data are particularly well-suited for studying the effect of government aid on refugees. Using rich micro-census data collected in West Germany in 1971, we can identify refugees directly rather than relying on country of origin as an indicator of "likely refugees," as is typically done in this context.[4] In addition, we observe the pool of applicants. We know eligibility for government aid on the individual level, and we know the age and year of arrival in the FRG as well as a range of outcome variables in 1971.

The refugee-targeted government support programs were implemented in addition to the FRG's welfare and social security system at the time and were intended to compensate for the hardship of the refugee experience. The supports - including cash, eligibility for education allowance, job-search assistance, access to credit for businesses, and access to real estate loans - sought to improve the refugees' chances of economic and social integration.

Interestingly, the East Germans fleeing to the FRG represented various socio-economic backgrounds, and overall, compare well with the West German population at the time. Certain groups are overrepresented, including families with fathers who were self-employed, who were farmers, or who had a university degree. This occurred because the oppression by the GDR government was rooted in communist ideology aiming at increasing social equality and forming socialist personalities. To achieve that goal, the regime used parents' education levels and occupations to identify families that needed intervention and re-education.

This specific setting allows us to bypass factors that often distort the integration of migrants, among them language barriers, mismatch of educational and vocational degrees, and legal issues related to residence or work permits. While this might constrain the one-to-one generalizability of results to other (more typical) groups of refugees when they arrive as adults, this is less of a concern when focusing on child refugees since they learn the host country language quite fast and, depending on their age at arrival, are readily integrated into the host country educational system. However, parents' obstacles to integration, including proficiency in the host country language, might matter for the outcomes of children. The analysis of our

[4] See for example Borjas (2000), Cortes (2004), and LoPalo (2018).

specific group of refugee children allows us to pin down the effects of government aid on families who must start anew - with the involuntary displacement, economic hardship, and social and psychological consequences that might be involved - while abstracting from other parental characteristics that potentially impede integration. The latter include host country language deficiencies, educational mismatch, and certain legal restrictions.

For refugees migrating as young adults (age-at-migration: 15-24), we find that immediate eligibility for government aid significantly improved the likelihood that they completed a higher level of education by 1971. Each additional potential year of aid-eligibility increases the probability of graduating from university by 1.2 percentage points, making those at the 75th percentile of the treatment distribution 6 percentage points more like to graduate from university than those at the 25th percentile. This is a large effect: only 14.2 percent of West German men aged 20-50 in 1971 had graduated from university. In line with this effect on human capital accumulation, these refugees also had higher-status jobs in 1971 and higher net monthly incomes.

Interestingly, these findings do not apply to aid-eligible refugees who migrated as children (age-at-migration: 1-14). For these refugees, schooling was compulsory, so they were readily integrated in the new host country educational institutions, and they faced no trade-off between investing in human capital or start working right away.

Our results suggest that government spending makes a difference in situations where parents have very few resources at their disposal and, therefore, are highly liquidity constrained. If the children in those families are at ages in which they face trade-offs between investing in human capital and starting to work, government aid that alleviates the family's financial constraints leads those children to invest more in human capital, and, ultimately, be more successful in the labor market. We find this to be true even when parents are highly skilled and even in a system where education is tuition-free.

Beyond the studies referred to above, we build on scholarship that analyzed the same historical setting that we use. The GDR refugees we focus on are the foundation of the social ties between East and West Germans analyzed by Burchardi and Hassan (2013) and Dorner et al. (2016) in the context of German reunification. Lüttinger (1986, 1989) provides a detailed descriptive comparison of native West Germans, people expelled from Central and Eastern Europe, and GDR refugees, finding that qualification levels and occupational status were higher among GDR refugees. Also related are studies on expellees from Central and Eastern European countries, groups that have received more attention in the economic literature than the GDR refugees. Falck et al. (2012) suggest that expellees did not improve their situation relative to West German natives, while Bauer et al. (2013) find that they experienced

economic penalties (except for those who moved from agriculture into other sectors). Braun and Kvasnicka (2014) analyze the effect on sectoral change and productivity, while Braun and Mahmoud (2014) show that the influx of expellees decreased native employment in the short term. Note, however, that the influx of expellees from Central and Eastern European countries to West Germany was completed before 1950, whereas the influx of GDR refugees had its peak after 1950 and continued until the Berlin wall was built in 1961.

This paper is structured as follows: Section 3.2 explains the historical background; Section 3.3 describes the data, samples, and main variables; Section 3.4 presents the empirical framework and Section 3.5 includes the analyses and results. We conclude in Section 3.6.

3.2 Historical Background

3.2.1 General Background

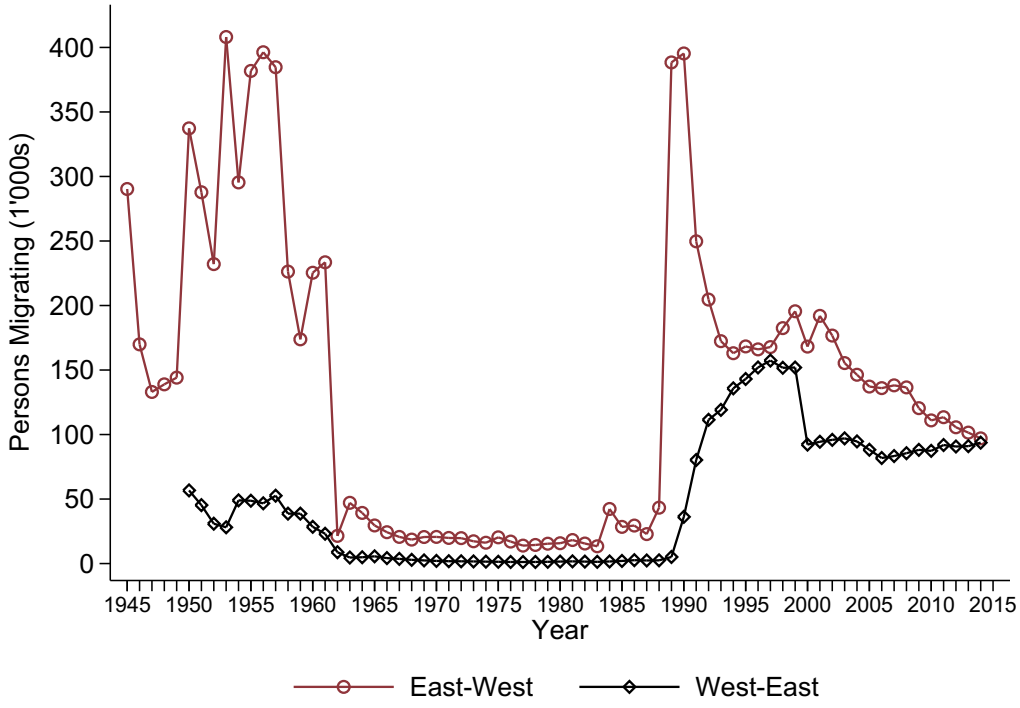
After the defeat of Nazi Germany in 1945, the Potsdam Treaty divided the remaining German territory west of the Oder-Neisse line into four occupation zones under the United States, the United Kingdom, France, and the Soviet Union. In 1949, the Soviet Occupation Zone in the east of Germany became the German Democratic Republic (GDR), organized as a communist state with a planned economy. The three other zones in the west became the Federal Republic of Germany (FRG), founded as a democracy with a market economy.

Even though there was no free movement between the different occupation zones, the authorities could not prevent the mass migration from the East to the West that we focus on in this paper. Between 1944 and 1961, at least 3.6 million refugees (*Flüchtlinge* or *Zuwanderer* in German) from East Germany are estimated to have arrived in West Germany.[5] This stream of East Germans to West Germany is depicted in Figure 3.1, together with the migration between East and West Germany until 2015. The figure illustrates this migration in net terms (East-West migration minus West-East migration). At the height of the outflow in 1954, for example, 400,000 East Germans fled to the West. The historical migration from East to West is even more important than the migration experience after the fall of the Berlin Wall in 1989.[6] To get a sense of the magnitude, the West German population was 39 million in 1939 (thus, the GDR refugees represented 9 percent of the West German population at the time) and the East German population was 17 million in 1939 (i.e. an estimated 21 percent of the

[5]This estimate is based on the Census of 1961. It excludes all expellees from Eastern Europe who moved to West Germany via the GDR. If Eastern European expellees who left the GDR between 1950 and 1961 (and likely did so because of the communist regime) are included, the estimated number of refugees increases to 4.5 million. (Authors' compilation is based on Heidemeyer (1994, pp. 43ff.).)

[6]Hunt (2006), Fuchs-Schündeln and Schündeln (2009), and Prantl and Spitz-Oener (2014) focus on the latter.

Figure 3.1: Migration between East and West Germany, in Thousands, 1944/45 to 2014



Sources: *1945-1949, East to West*: Based on retrospective information from the Census of 1961, excluding Eastern European expellees who arrived in West Germany via the Soviet Occupation Zone, and taken from Heidemeyer (1994, p. 44). "1945" combines figures for both 1944 and 1945. *1950-61, both directions*: Mobility statistics provided by the Federal Statistical Office, obtained from Heidemeyer (1994, p. 45). *1962-2015, both directions*: Mobility statistics provided by the Federal Statistical Office upon request. Note that all data from 1950 onward stem from registration offices.

East German population fled between 1944 and 1961).[7]

The historical literature names three main drivers that pushed people to escape the GDR (see, for example Heidemeyer, 1994; Van Melis, 2006). One group of refugees were regime opponents who fled for directly political reasons. These included members of the Protestant church youth organization, the so-called *Junge Gemeinde* (Young Parish), which was oppressed by the communist regime. In this category were also people forced to work in uranium mining or for the East German police.

A second driver was the centrally planned communist system's economic oppression and downgrading. Farmers and agricultural workers, for example, fled as a consequence of the expropriation and reorganization of their farms for use as agricultural production cooperatives, corresponding to Soviet *kolkhoz*. Expropriations also affected owners of industrial firms. Shopkeepers and self-employed skilled trades workers faced discriminatory taxation and

[7]These population figures refer to the territories of the later FRG and GDR, respectively; see Federal Statistical Office (1952, p. 12) and Governmental Central Office for Statistics (1955, p. 8).

restricted access to markets for inputs. The academic elite and technical specialists left because their children were blocked from higher education and from freely choosing their occupations.

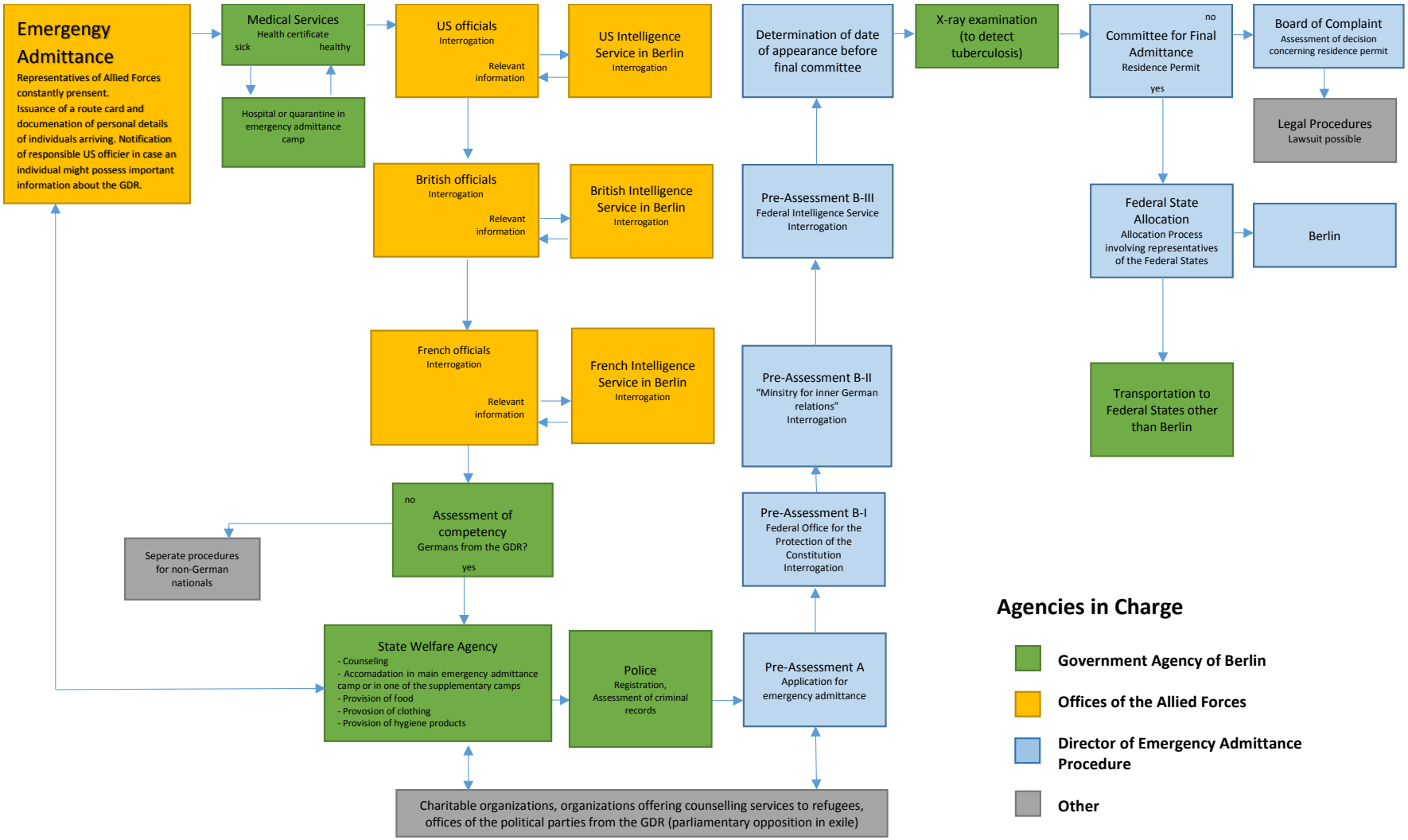
More general situations, including shortages of goods and limited housing opportunities, prompted the other group of refugees to leave the GDR. Reunification with family members in the West was also a reason.[8]

From today's perspective, persons who left the GDR and moved to West Germany can, in large part, be viewed as refugees. Had the regime been a different one, they would have stayed. They felt forced to flee the GDR, escaping secretly and illegally, while leaving behind most of their belongings, their property, and their social network. They risked monetary penalties and imprisonment, and they exposed family members left behind to punishment. In addition, upon arrival in West Germany, GDR refugees were first confined to refugee camps, from which they were then allocated to the different West German regions (Kimmel, 2005; Van Melis, 2006; Limbach, 2011). While during the immediate first years after the war there were no common procedures in the different occupation zones, the refugee management process became more and more harmonized and standardized across West Germany, with the Emergency Reception Procedures (*Notaufnahmegesetz*) becoming the FRG-wide legal basis from September 1950 to June 1990. Figure 3.2 includes a schematic overview of the screening and examination process the Emergency Reception Procedures involved.

Starting in 1952, the communist regime stepped up efforts to deter migration. It established a very effective 5-kilometer-wide exclusion zone along the more than 1,000-kilometer-long border between West and East Germany, which was heavily fortified and patrolled by armed police. As a result, refugees had to cross into West Berlin via train after 1952. Before being allocated to the different West German regions (and flown there by plane), they lived in a refugee camp in West Berlin, where they were registered, examined and interrogated (see Figure 3.2, again). In 1961, that final path to the West was ultimately blocked by the erection of the Berlin Wall (see, for example Van Melis, 2006). Migration to West Germany was not possible again for 28 years, when the Berlin Wall fell, and communism collapsed.

[8] GDR refugees also included a group of spies and criminals which, despite its small size, received considerable attention in the West German debate (ibid, see also Ackermann, 1995).

Figure 3.2: Schematic Overview of the Emergence Reception Procedure in Marienfelde in 1960



Source: Replication from Kimmel (2005, p. 121)

3.2.2 State of the West German Economy and Welfare State

From the end of World War II through the beginning of the 1970s, West German society was shaped by two phenomena. The first was the key societal challenge brought by unemployment, housing shortages, the mass in-migration of expellees and GDR refugees, and the re-integration of war victims. The policy response in the immediate years after the war included food stamps, massive investments in social housing programs and, as we explain in more detail in the next sections, programs targeting expellees and GDR refugees (Schulz, 2005a; Löffler, 2007).

The second phenomenon was the exceptional GDP growth in the 1950s and 1960s. Between 1950 and 1960, annual GDP increased by 127 percent; between 1960 and 1970, it swelled another 53 percent (Lampert and Althammer, 2001, p. 88). West Germany was able to overcome the hardship of the immediate years after 1945 relatively fast so that increased real incomes reached broad levels of the population and inflation rates were stable and moderate. By the end of the 1950s, West Germany had achieved full employment (Schulz, 2005a; Löffler, 2007). This implies that the socio-economic integration of GDR refugees in the 1950s and 1960s occurred against the backdrop of particularly favorable economic development.

West Germany was organized as a social market economy that combined liberal (but regulated) markets with a comprehensive welfare state supported by two pillars (Esping-Andersen, 1990): the social security system and welfare benefits. The social security system includes mandatory health, accident, pension, and unemployment insurance schemes. Social security coverage is tied to employment and co-financed by employers and employees. Contributions are determined as a function of employees' wages, and benefits depend on prior contributions (Schulz, 2005b). Welfare benefits, the second pillar, are paid to individuals or families in need who are otherwise uninsured and do not have sufficient personal financial means. Welfare benefits are funded through taxes and include a monthly allowance that covers basic costs. Although the monthly allowance previously existed, it became an enforceable right only in 1962, the same year that welfare benefits were introduced for persons experiencing illness, depending on care, or facing other difficult circumstances (Boldorf, 2007).

At that time, the male breadwinner model was the norm in West Germany. Married women - especially mothers - tended to be housewives who relied on their husbands' incomes. Female employment increased only gradually. Among married women below the age of 65, only 26 percent of women worked outside the home in 1950. By 1961, that figure rose to 37 percent; in 1970, it was 41 percent (Schulz, 2005b, p. 43). With social security coverage tied

to employment, West German housewives were disadvantaged.[9] To partially compensate for this gender inequality and to fight poverty among families with many children, child benefits were introduced in 1955.[10] These benefits came in tandem with income tax deductions for families (Münch, 2005, 2007).

All refugees from the GDR had access to social security and welfare benefits, irrespective of legal status. In 1953, they were integrated into the West German health, accident, and pension insurance schemes and, in 1956, into the unemployment insurance scheme. This process involved acknowledgment of social security entitlements based on periods of employment in the GDR (Heidemeyer, 1994, pp. 232-241). They were also entitled to child benefits and family tax deductions (Nahm, 1967, p. 8).

3.2.3 Criteria Determining GDR Refugees' Eligibility for Additional Benefits

Beyond the social security and welfare benefits, large-scale redistribution schemes were implemented in West Germany to partially compensate those who incurred losses during World War II. From the early 1950s through 1966, some 63 billion Deutsche Mark were redistributed under the Equalization of Burdens Act. This remains one of the biggest economic and financial transactions in German history, with roughly 25 percent of the 1966 GDP redistributed over about 15 years.[11] Funding came through designated taxes (Nahm (1967, p. 20), Abelshauser (2011, p. 335)).

There was a broad political consensus that expellees from Eastern and Central Europe should benefit from the redistribution programs (Werber et al., 1954; Heidemeyer, 1994). The question whether GDR refugees should also benefit sparked a major and controversial political debate. Until the Berlin Wall was built in 1961, the debate revolved around the question of whether East Germans were genuine refugees, as discussed before. In this context, influential members of the governing Christian Democratic Party opposed the inclusion of GDR refugees in the redistribution programs. Their underlying motive was to discourage further migration from East Germany, although program costs were also a consideration (Heidemeyer, 1994; Ackermann, 1995).[12]

[9] Women and children are, however, entitled to health insurance through their husbands' or fathers' insurance. Within the pension insurance scheme, widows receive surviving dependents' pensions.

[10] Child benefits were paid per child from the third child onward and, after 1961, from the second child onward. From 1955 through 1965, the child benefits were raised multiple times. In principle, the benefits were designed at a flat-rate, but they were reduced for families with incomes exceeding a certain threshold. Initially, the child benefits were financed by employers and the self-employed. In later years, they were funded through taxes (Münch, 2005, 2007).

[11] West German annual GDP in 1966 was 249 billion deutsche mark. The figures cited here are in 1966 prices.

[12] The parliamentary opposition, the Social Democrats, advocated a more liberal response to the refugee

In the end, only a subgroup of East German migrants was given status on par with the expellees and made eligible for financial and other aid that addressed their specific needs as refugees. And that was true only from 1953 forward. Those "political GDR refugees" received so-called "C-status" that was documented in refugee-passports, and that is the legal background of the government aid we analyze here.[13] C-status was decided in refugee offices at the regional level. Refugees who migrated before 1953 could apply retroactively. Children were automatically assigned the same status as their parents (Werber et al., 1954; Ackermann, 1995).

3.2.4 Benefits for Political GDR-Refugees

GDR migrants who were acknowledged as political refugees became eligible for an additional set of governmental programs (Lüder, 1957, for an overview see our compilation in Appendix Table B.1), including cash benefits and access to loans. Lump-sum cash benefits were paid for the purchase of household goods and personal effects that might have been lost. A monthly cash allowance was extended to refugees and their relatives who wanted to complete a vocational qualification or university degree but lacked the financial means. Loans, meanwhile, were provided for the purchase of real estate used for private purposes and for professionals, farmers, and business owners who had lost their capital. Rent-controlled apartments were available through yet another program.[14] There also existed the possibility of a publicly subsidized job (Lüder, 1957; Nahm, 1967). Indeed, various measures were intended to boost the refugees' labor market integration. Among the target groups were farmers or agricultural workers (via affordable loans and cash benefits), the self-employed (via affordable loans, debt guarantees, co-partnerships, tax cuts, cash benefits, and other privileges), and employees (via privileged treatment by employment agencies, privileged re-entry into previous occupations, and privileged access to apprenticeships). Moreover, the benefits included generous regulations governing past debts (Werber et al., 1954; Lüder, 1957).

In other words, the refugee-specific aid granted political refugees access to the social security and welfare system of the FRG while also making them eligible for additional programs that aimed to provide partial compensation for the losses they had incurred. Most components of the refugee-specific aid alleviated current needs and cannot be characterized

inflow but lacked the political power to put it into action.

[13] The specific law is the Federal Expellee Law (*Bundesvertriebenengesetz, BVFG*) of 1953.

[14] Two laws formed the basis for the benefits, the Hardship Fund of the Equalization of Burdens Act (*Lastenausgleichsgesetz, LAG*) and the Federal Expellee Law (*Bundesvertriebenengesetz, BVFG*). Until 1966, 2.5 billion deutsche mark were distributed under the Hardship Fund (Nahm, 1967, p. 32)

as direct investments in refugee children. However, there is one important exception: student allowances. Student allowances may allow young adults who face trade-offs between investing in human capital and starting to work to overcome the financial constraints that make their decisions myopic. Overall, and in contrast with children, young adults might be more vulnerable to an absence of refugee-specific government aid since, again, they are less naturally linked to the educational institutions of the host country. Economic necessity combined with a lack of government assistance may preclude these young adult refugees from pursuing higher education, which might have been beneficial for their long-term well-being.

Policymakers sought to improve the refugees' socio-economic status while moving toward equality of opportunities between refugees and West German natives (Werber et al., 1954; Heidemeyer, 1994). Accordingly, eligibility for benefits ended once the individual recipients reached a sufficient degree of social and economic integration. East German migrants who were not acknowledged as political refugees were excluded from these comprehensive programs.

The benefits paid to political refugees were economically significant. For example, adolescents and young adults willing to tolerate modest living standards were able to be full-time university students or apprentices thanks to the monthly education or vocational training allowance (Gillner, 1955).

3.2.5 West German Educational System

Primary, secondary, and tertiary education is generally free of charge in Germany, implying that there is no tuition. In the time span relevant to our study, compulsory schooling covered eight years in a system that had three school "tracks:" the lowest track that ended after eight grade (and encompassed the majority of students), an intermediate track to which student switched after fourth grade, and the college-bound Gymnasium track. In the early years of the Federal Republic of Germany, few students moved to the intermediate school track (3.3 percent in 1951) or to the highest school track, the Gymnasium (9.7 percent in 1951), which qualified students for direct university admission (the remaining 87 percent attended the lowest school track). In the years that followed, enrollment in the intermediate and Gymnasium tracks rose (Schulz, 2005a, p. 60).

As part of this general expansion in education, a national student-aid program was introduced in the winter term of 1957/58. Students with good academic records but without the financial means to attend university were paid a monthly education allowance so they could study. Half of the allowance was stipend-based, and half was financed through student loans (Anweiler, 2005). In 1971, the year when our data were sampled, 14.2 percent of West

German men aged 20 to 50 had obtained a university degree, 63.7 an apprenticeship degree, and 22.0 percent had no formal qualification.[15]

3.3 Data, Sample, and Definition of Main Variables

3.3.1 Data

Our analysis is based on the Supplementary Microcensus of 1971, the so-called *Mikrozensuszusatzerhebung (MZU 1971)*, conducted by the German Federal Statistical Office in April 1971. It is a 1 percent representative sample of the West German native population aged 15 and older, and its aim was to elicit economic and social transformations in post-war Germany. Respondents were required by law to participate in the survey (for a detailed description, see Tegtmeyer, 1979).

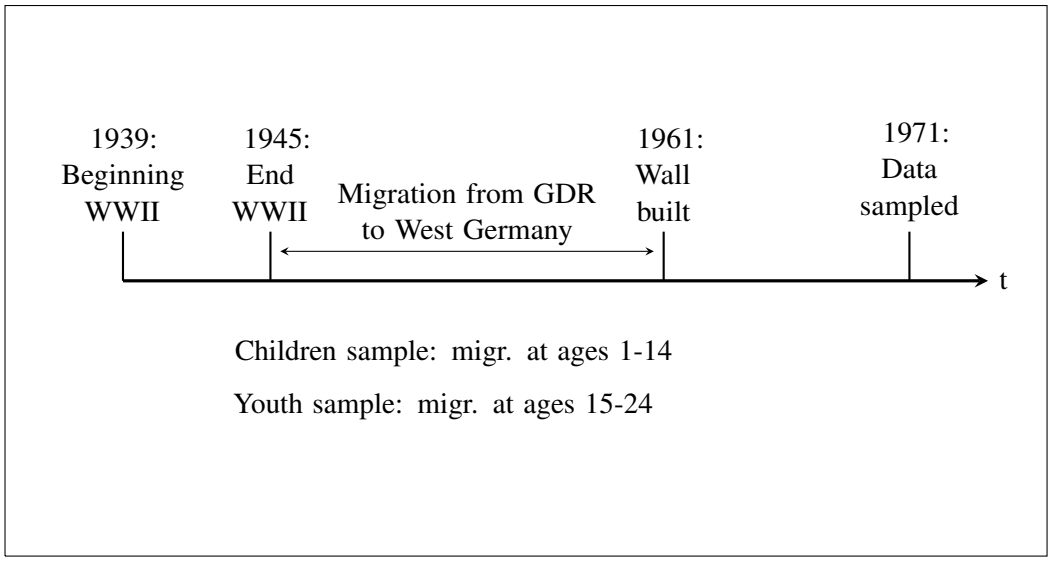
For the purposes of our study, the MZU 1971 has three major advantages. First, these data contain information on a person's region of origin and population group, allowing us to identify refugees from the GDR. We also know their age at arrival and the year of arrival in the West; and we can see whether former GDR citizens were eligible for government aid. A second advantage of the data is its detailed parental background characteristics for refugees at age 15. This information, collected for all persons born in 1920 or later, includes the father's detailed occupational status (13 categories), the father's industry (16 categories), the father's qualification level (6 categories), and the mother's qualification level (6 categories).[16] They were the most important characteristics used in distinguishing political refugees from economic refugees. The third advantage is that the data provide detailed information on individuals' socio-economic outcomes in 1971. These variables refer to a person's education, employment, type of employment, and income. Overall, the MZU 1971 is an ideal data source for studying the impact of governmental support programs on child and young adult refugees' economic success.

The MZU 1971 has a clustered survey design. Throughout the analysis, we account for

[15] Authors' estimates based on the data described in the next section. The corresponding shares for women are 7.8 percent (university graduates), 63.7 percent (women with completed apprenticeship training), and 49.0 percent (no formal qualification).

[16] The survey also includes mothers' occupational status and industry at age 15. However, in many cases mothers dropped out of the labor force and this information is either missing or not very meaningful. Therefore, we focus on fathers' occupational status and industry. The occupational status variable is influenced by Max Weber's concept of social stratification (Lüttinger, 1989, p. 73). On the one hand, the variable divides occupational status into horizontal categories (for example, by distinguishing among the self-employed, the employed, civil servants, workers, etc.). On the other hand, the variable elicits status differences within these categories (for example, by distinguishing among low, medium, and high-level civil employees). Because this status variable is crucial for our analysis, our main samples exclude all observations for which the father's occupation status was missing. We return to this restriction later when we assess the robustness of our results.

Figure 3.3: Definition of Samples and Historical Time Line



a potential dependence of observations within the same sampling units by clustering our standard errors accordingly (cf. Deaton, 1997, p. 71ff.).[17]

3.3.2 Samples

Our analysis is based on refugees from East Germany who migrated to West Germany before the Berlin wall was built in 1961.[18] We begin with individuals migrating from 1946 (bypassing 1945, which was marked by the turmoil and chaos of the end of the war). In addition, we restrict our analysis to men. Our goal is an examination of outcomes that capture refugees' economic success in 1971. Since the male breadwinner model dominated in West Germany and women - especially mothers - dropped out of the labor force, it is reasonable to focus on men. Indeed, only 55 percent of East German women who migrated as young adults (at ages 15 to 24) and 50 percent of women who migrated as children (at ages 1 to 15) were employed in 1971.[19]

[17] In the first sampling stage, 10 percent of all sampling districts were randomly selected. In the second stage, 10 percent of the population aged 15 and older was randomly selected within the sampled districts. Since sampling districts were relatively small, there are several hundred clusters (for details see Schimpl-Neimanns, 2016).

[18] We exclude former expellees from Eastern European territories who arrived in West Germany via East Germany. These former expellees form a distinct group that was forced to migrate twice and, hence, was entitled to different governmental support programs than East German refugees who migrated only once. By only including persons living at their main residence, we also impose restrictions that make our sample representative. Furthermore, we exclude observations classified as supplemental and duplicated observations (for details see Schimpl-Neimanns, 2016).

[19] Authors' calculations. One consequence of the low female employment rates was that significant shares of women did not report important economic indicators like their income.

Since the economic success of migrants likely depends on the age at which a refugee arrives in the country of destination, we focus our analysis on two different samples. The sample definitions and the historical time line are summarized in Figure 3.3. We call our samples the children sample and the youth sample (=young adults). The children sample consists of refugees who arrived in West Germany at ages 1 to 14, a time frame in which they are subject to compulsory school attendance. (School was mandatory through eighth grade, when students were typically 15 years old.) Therefore, the children sample comprises first generation migrants who arrived in the destination country before they left high school. We exclude those who were born in West Germany because we want to make sure that they were born in the GDR and experienced the flight experience.

The youth sample, meanwhile, includes refugees who migrated at ages 15 to 24 and, because of their age at arrival in West Germany, were not obligated to attend school. They had to decide whether to continue going to school, pursue an apprenticeship degree, attend university, or immediately search for a job.

3.3.3 Definition of Main Variables

To capture medium-term outcomes of refugee children and young adults in 1971, we focus on two sets of outcome variables. First, we place emphasis on variables capturing educational attainment. This is due to the fact that the education allowance was the component of the refugee aid program targeting refugees migrating at young ages. Specifically, we assess how refugee-specific aid impacted the likelihood of obtaining a high qualification (i.e., graduating from university), a medium qualification (i.e., completing an apprenticeship training degree), or a low qualification (i.e., not obtaining a formal qualification). For the children sample, we also assess whether refugees are in the process of receiving higher education or of completing an apprenticeship training degree.

Second, we complement the analysis of educational attainment by looking at a set of additional economic outcome variables. In terms of employment outcomes, we include an indicator variable for being employed, as well as a variable capturing job quality. The latter is an indicator variable for a person employed as a high-level civil servant, high-level employee, or elite worker (i.e., a worker with managerial responsibilities). Furthermore, we assess individuals' total monthly net income from all income sources. The income variable is not to be confused with (labor market) wages, but it does constitute a proxy variable for socio-economic status.

In Table 3.1, we report summary statistics for the children and the youth samples. About 57 percent of our overall sample belongs to the youth sample and about 43 percent to the

Table 3.1: Summary Statistics for Main Variables

Variable (Unit of Measurement)	Children Sample (Age-at-Arrival: 1-14)	Youth Sample (Age-at-Arrival: 15-24)
AID (0/1)	0.34	0.21
Potential Years of Exposure (years)	5.34	3.23
Std. Dev.	(7.67)	(6.39)
25% percentile	0	0
75% percentile	14	0
90% percentile	18	17
Age in 1971 (years)	25.79	37.77
Std. Dev.	(5.83)	(5.06)
High Qualification (0/1)	0.13	0.21
Medium Qualification (0/1)	0.53	0.62
No Qualification (0/1)	0.34	0.17
In Education (0/1)	0.15	0.01
In Apprenticeship Training (0/1)	0.07	0
Employed (0/1)	0.84	0.98
High status job (0/1)	0.17	0.22
Ind. total net monthly income (DM)	987.74	1253.12
Std. Dev.	(464.75)	(423.00)
N	1,151	1,551

Notes: The table contains summary statistics for the children and youth samples, with standard deviations in parentheses (where indicated). All variables refer to 1971. See Appendix Table B.2 for a detailed description of some of the variables. The sample includes men migrating from the GDR between 1946 and 1961 at ages 1 to 14 (children sample) and at ages 15 to 24 (youth sample).

children sample. Among refugees arriving as young adults, 21 percent were eligible for government aid, compared with 34 percent among refugees arriving as children. The average age of the children sample is 26 in 1971; it is 38 for the youth sample. Table 3.1 also displays summary statistics of the outcome variables that we analyze in the following section. A detailed description of additional variables is included in Appendix Table B.2.

3.4 Empirical Specifications and Identification Strategy

We are interested in the effect of aid-eligibility on refugee children and young adults' economic success later in life. This effect is difficult to measure as those eligible for aid are both self-selected and screened by the local authorities. In our empirical approach we combine several strategies. First, as we focus on GDR refugees, and given how the refugees'

admission in West Germany was managed by the authorities, our analyses consider the pool of aid applicants, and we control for the most important characteristics that the local authorities used to select aid-eligible persons from the pool of applicants. In addition, we take advantage of the fact that the refugee-targeted aid was only available after 1953.

We restrict our analyses to GDR refugees who all wanted to be legally acknowledged to build their new life, that is, they reflect the applicant pool. The applicants were thoroughly screened, in a specific refugee screening process that involved West German authorities and those of the Allied countries.

All refugees were first hosted in refugee camps, rigorously interrogated by both West German authorities and by the Allies (represented in general by members of the different countries' intelligence institutions). The whole screening process took several weeks, and all refugees had to go through it. At the end, there was a committee with three members who took all stages of the screening process into account when summarizing the escape motives and making the decision about the legal status of the refugees. So, we are comparing two groups of people who - by fleeing the GDR - have both strongly indicated that they want to build a new life in West Germany permanently. That is, both groups had a strong incentive to be legally acknowledged, independently from the additional refugee-targeted aid. While the application for government aid was nominally voluntary, once refugees had gone through the screening process, it was largely a minor formality.

The data analyzed here includes the most important characteristics that the authorities used to screen refugee families, namely parental background characteristics, allowing us to reduce the selection bias introduced by the screening process of the authorities.

Against this background, and considering that the refugee-targeted aid was only introduced in 1953, our basic specification takes the following form:

$$Y_i = \alpha + \beta_1 AID_i + \beta_2 T_i + \beta_3 AID_i * T_i + \beta_4 X_i^{basic} + \beta_5 X_i^{par} + \varepsilon_i. \tag{3.1}$$

Y_i captures medium-term economic outcomes of refugees measured in 1971. Specifically, Y_i includes variables on education outcomes, employment outcomes and income. Because age-at-arrival determines the integration of young refugees into the West German education system, we separately analyze two different groups of GDR refugees: children (age-at-arrival: 1-14) and young adults (age-at-arrival: 15-24). See previous discussions.

The variable AID_i is a dummy variable indicating refugees' eligibility for government aid at any point after arrival, T_i is a dummy variable for having arrived in West Germany in 1953, or later, and $AID_i * T_i$ is an interaction term indicating GDR refugees who were eligible for aid and arrived in 1953, or thereafter. This specification reflects the basic institutional details

of the government aid program that we analyze. The program was introduced in 1953, which rationalizes the separation of those who arrived before 1953, and those who arrived thereafter. At the same time, refugees who arrived before 1953 and who were acknowledged as legal refugees were also eligible for aid, that is AID_i has values of one and zero for refugees arriving between 1946 and 1961.

The coefficient β_1 captures the average difference between those who were eligible for government aid and those who were not, whereas coefficient β_3 on the interaction term $AID_i * T_i$ indicates whether this average effect is systematically different for those who arrived after 1953.

Conceptually, we differentiate the effects of those who became aid eligible immediately after arrival $(T_i = 1)$, from those who arrived in the same "age-at-arrival" range but became eligible later (i.e. when they were older, $T_i = 0$). For example, refugee children who were younger than 15 at arrival in West Germany and arrived before 1953 were on average 11.8 years old in 1953, when the refugee-targeted aid was introduced. The counterpart of children who were also younger than 15 at arrival, but arrived in 1953, or later, were 4.2 years old on average when they became eligible for the refugee-specific aid (note that the average age at arrival for children, who arrived below the age of 15, was 7.6 for both those who arrived before 1953 and those who arrived thereafter).

Analogously, young adults who were between 15 and 24 at arrival and arrived before 1953 were on average 24.3 years old in 1953, when the refugee-targeted aid was introduced, whereas those who arrived in 1953, or later, were on average 19.9 years old when they became eligible (even though those who arrived before 1953 and those who arrived thereafter were on average about 20 years old when they arrived).

In the basic specification, we address the age difference in two ways. First, T_i captures level differences in the outcome variable between the early and late arrivals, including differences in outcomes that arise because of differences in age when they became eligible. T_i also picks up unobserved heterogeneity between the early and late arrivals if this unobserved heterogeneity is time-invariant.

In addition, we take the age difference into account by controlling for the age of individuals in 1971. Specifically, X_i^{basic} includes age in 1971 and its square. It also includes the year of migration which is important as it accounts for aggregate, year-specific differences between 1946 and 1961 that may affect the outcome variables in 1971 for all individuals in the same way.

X_i^{par} in Specification (3.1) includes the refugees' parental background characteristics, the most important observable characteristics that the authorities used to screen the refugees, and

which allow us to reduce selection bias introduced by the screening process of the authorities. As described earlier, in West Germany the authorities focused on background characteristics of the adults in a family because those are the characteristics that the GDR regime used to identify potential "class enemies." So, both sides used the same "profiling" characteristics to predict likely political refugees (who became eligible for government aid) as opposed to likely economic refugee (who did not become eligible for government aid). The advantage of our data is that we do observe parental background characteristics when the refugee children and young adults were 15 years old, among them the father's occupational status (13 categories), father's industry (16 categories), and father and mother's qualification levels (6 categories, respectively).[20] Of course, those characteristics are important control variables by themselves, given our research question.

Up to this point, we have treated AID_i as a dummy variable. But the institutional set up of our experiment allows us to go one step further and use differences in treatment dose as the identifying variation. The coefficient on the dummy variable specification represents a weighted average of the per-unit causal effect along the length of the causal response function. As we will show below, there is large variation in the number of potential years of aid-eligibility, making a specification that exploits this variation more persuasive.

When constructing the treatment dose measure, we exploit the fact that GDR refugees differed in terms of when they arrived, in combination with the fact that refugee-targeted government support programs were only implemented for the GDR refugees in 1953.

We take explicitly into account that refugees' year of arrival and therefore the potential number of years of exposure to aid-eligibility varied within the "age-at-arrival" groups. Specifically, we define a variable capturing potential years of exposure (EXP_i) to government aid as follows:

$$EXP_i = \begin{cases} 0 & \text{if } AID_i = 0 \\ 1971 - year\_migration_i & \text{if } AID_i = 1 \text{ and } year\_migration_i > 1953 \\ 1971 - 1953 & \text{if } AID_i = 1 \text{ and } year\_migration_i \leq 1953 \end{cases} \tag{3.2}$$

That is, for those who are not eligible, EXP_i is still zero. However, for those who arrived after the introduction of the refugee-targeted government aid in 1953, the potential number of years of eligibility is 1971 (the year of the survey and in which we measure our outcomes) minus the year of arrival ($year\_migration_i$) in West Germany (which is also equal to the difference in age in 1971 and age at arrival). For those who arrived before 1953, aid-eligibility

[20] See Appendix Table B.2 for a detailed definition of the variables included in X_i^{par}.

Table 3.2: Summary Statistics for Unconditional Potential Years of Exposure (EXP), by Arrival Sample and Time

	(1) Mean	(2) Std. Dev.	(3) Median	(4) 75% Pctile	(5) 90% Pctile	(6) 95% Pctile	(7) N
Children Sample, Age at Arrival: 1-14							
Before 1953							
EXP	4.92	8.03	0	18	18	18	542
1953 and later							
EXP	5.72	7.32	0	13	17	18	609
Youth Sample, Age at Arrival: 15-24							
Before 1953							
EXP	3.11	6.81	0	0	18	18	677
1953 and later							
EXP	3.33	6.04	0	0	14	16	874

Notes: The table contains unconditional summary statistics for the variable "Potential Years of Exposure" (EXP) by arrival time and arrival sample. The sample includes men migrating from the GDR between 1946 and 1961 at ages 1 to 14 (children sample) and at ages 15 to 24 (youth sample).

only started after 1953 when they had already lived in West Germany for a couple of years and were older compared to their age-at-arrival cohort that arrived in 1953, or later (see the example above when we discuss the role of T_i). While aid-eligibility did not start for this group immediately after arrival in West Germany, it potentially lasted for 18 years, from 1953 to 1971.

Overall, we find considerable variation in treatment dose. As Table 3.1 shows, in the children sample, the average potential years of exposure is 5.3 (std. dev. 7.7); in the young adult sample, it is 3.2 (std. dev. 6.4). Tables 3.2 and 3.3 show further details of this variable, both unconditionally (Table 3.2) and conditionally on aid-eligibility (Table 3.3). As is clear from those statistics, those who arrived earlier had more years of potential aid-eligibility but it started when they were older. Table B.3 in the Appendix shows in detail how EXP_i varies by "age in 1971."

Against this background, we estimate the following model:

$$Y_i = a + \theta_1 EXP_i + \theta_2 T_i + \theta_3 EXP_i * T_i + \theta_4 X_i^{basic} + \theta_5 X_i^{par} + \varepsilon_i. \tag{3.3}$$

Our coefficients of main interest are θ_1 and θ_3, the average impact of an additional potential year of aid-eligibility on the outcome variable (θ_1) and the difference in the effect for those aid-eligible who arrived in 1953, or later (θ_3). All other variables in Specification (3.3)

Table 3.3: Summary Statistics for Potential Years of Exposure Conditional on Aid-Eligibility, by Arrival Sample and Time

	(1) Mean	(2) Std. Dev.	(3) 5% Pctile	(4) 25% Pctile	(5) Median	(6) 75% Pctile	(7) 95% Pctile	(8) N
Children Sample, Age at Arrival: 1-14								
Before 1953								
EXP	18	0	18	18	18	18	18	148
1953 and later								
EXP	14.5	2.9	10	11	15	17	18	240
Youth Sample, Age at Arrival: 15-24								
Before 1953								
EXP	18	0	18	18	18	18	18	117
1953 and later								
EXP	13.8	2.6	10	11	14	16	18	211

Notes: The table contains summary statistics for the variable "Potential Years of Exposure" (EXP) by arrival time and arrival sample, conditional on being aid-eligible. The sample includes men migrating from the GDR between 1946 and 1961 at ages 1 to 14 (children sample) and at ages 15 to 24 (youth sample).

are defined as in Specification (3.1), and the interpretation of estimated coefficients is as discussed before.

Given the variation in the data we use for effect identification in all specifications, it is important to examine whether there are systematic differences between GDR refugees across age-at-arrival and year of arrival. To do so, we show empirical evidence that suggests that there is no systematic difference between the key observable characteristics of those who arrived before 1953 - when neither the refugees nor people involved in the screening process knew that the refugee-targeted government aid program would be introduced - and those who arrived in 1953, or later.

First, we focus on age-at-arrival differences. It could for example be that the authorities who screened the refugee families systematically treated families with kids in certain age ranges differently, because, for example, the access to student allowances was important at the time for the family. Table 3.4 shows mean age at arrival for both the children and youth samples. Furthermore, we distinguish between "before 1953," the time before the refugee-specific aid was implemented and before invested parties knew about it, and 1953 onward. Had there been strategic sorting based on age at arrival, we would expect to observe changes in age at arrival by refugee status and over time. This is clearly not the case. For refugees of the children sample who received aid, the mean age at arrival before 1953 was 7.6 years (column 1(a)). It was virtually the same - 7.7 years - for those arriving in 1953

Table 3.4: Comparison of Age-at-Arrival and Father's Education Differences for Refugees who Arrived before 1953 and Thereafter

	(1)	(2)	(3)	(4)	(5)	(6)
	Children sample (Age-at-Arrival: 1-14)			Youth sample (Age-at-Arrival: 15-24)		
	Mean AID	Mean NO AID	Difference (2)-(1)	Mean AID	Mean NO AID	Difference (5)-(4)
(a) Arrival before 1953						
Age at arrival	7.6	7.7	0.1	19.8	20.1	0.3
N	148	394		117	560	
Father's education						
High qualif.	0.07	0.09	0.02	0.02	0.05	0.03
Med. qualif	0.66	0.72	0.06	0.60	0.60	0.00
No qualif.	0.27	0.19	-0.08*	0.38	0.35	-0.03
N	130	345		99	532	
(b) Arrival in 1953 or later						
Age at arrival	7.7	7.6	-0.1	19.9	19.9	0.0
N	240	369		211	663	
Father's education						
High qualif.	0.08	0.05	-0.03	0.08	0.05	-0.03
Med. qualif	0.73	0.73	0.00	0.65	0.69	0.03
No qualif.	0.19	0.22	0.03	0.27	0.26	-0.01
N	206	338		194	637	

Notes: The table refers to men who migrated from East to West Germany between 1946 and 1961. Columns (1) and (4) report mean characteristics for those who were eligible for aid. Columns (2) and (5) report the same statistic for those not eligible. Columns (3) and (6) report the differences in means. We conducted two-sided t-tests to evaluate the statistical significance of the difference, where one star * refers to significance at the 10 percent level. The absence of any stars (*) indicates that the differences are not statistically different at conventional levels.

and later (column 1(b)). Moreover, the age of arrival for those not receiving aid was not statistically different (column 3 (a) and (b)). The same applies to refugees arriving as young adults. Before 1953, the mean age at arrival for those receiving aid was 19.8 (column 4 (a)), while it was 19.9 from 1953 onward (column 4 (b)). Again, the mean age at arrival for those not receiving aid was not statistically different (column 6 (a) and (b)).

Similarly, we assess whether from 1953 onward there was strategic sorting into the program based on fathers' educational attainment. Moving through the table in the same way as we did for "age at arrival," there are no systematic differences. Only one difference is statistically significant at the 10 percent level: Fathers of refugee children who were aid-eligible and who arrived before 1953 were more likely to have no formal qualification level, suggesting that, if anything, there was negative selection into aid-eligibility based on

Figure 3.4: Share of Aid-Eligible Refugees, by Year of Arrival in West Germany

Sources: This figure shows the share of aid-eligible refugees who arrived in West Germany at ages 1-25, by year of arrival.

fathers' educational attainment.

Figure 3.4 shows the share of GDR refugees who were eligible for aid by year of arrival in West Germany. For arrival years before 1951, the yearly share was about 20 percent. It was higher for the years adjacent to 1953, in line with the GDR policies at the time (e.g., there was a wave of expropriations in agriculture), and mirroring the historical reasons for the introduction of the refugee-targeted aid in 1953. It was about 20 percent in the mid-1950s again, but increased for arrivals after 1957, reflecting again the changes in GDR policies (such as worsening conditions in schools and universities as well as for certain professional persons, see Ackermann, 1995).

We complement the previous analysis by examining the refugees' parental background characteristics in detail for each year of arrival in West Germany. We focus on the father's education, employment status, and industry; display the distributions of these variables for (1) all GDR refugees, (2) only those acknowledged as political refugees, and also show (3) the shares within each category who were acknowledged as political refugees (see Figures B.1, B.2, and B.3 in the Appendix). Two conclusions can be drawn from this analysis. First, there are fluctuations by year of arrival in West Germany.[21] Second, and importantly for our

[21] To give an example, consistent with the wave of expropriations in agriculture, the share of all GDR refugees whose fathers were farmers increased around 1953 and a high share of refugees whose fathers were farmers were acknowledged as political refugees around this year (see Figure B.2, Panels A to C). Similarly, and consistent

purposes, there are no patterns suggesting that the selectivity of persons acknowledged as political refugees systematically changed for those arriving from 1953 onwards. This further corroborates the variation we exploit for effect identification.

Finally, we should stress that our estimate on AID_i and EXP_i captures the effect on the outcomes of aid eligibility rather than aid receipt. That is, we are estimating an intention-to-treat effect. We see this as the relevant policy measure since policymakers may decide upon the offer of aid but not take-up. Moreover, the intention-to-treat effect is not confounded by potentially endogenous take-up of aid.

3.5 Results

3.5.1 Education and Other Economic Outcomes in 1971

Youth Sample (Age-at-Arrival: 15-24)

Each row of Table 3.5 includes the results for the outcome variables listed in the first column. Columns (1) to (3) show the results for β_1, β_2, and β_3 from Specification (3.1) when only the basic controls - age, its square, and year of migration to West Germany - are included in the specification; columns (4) to (6) show the respective results when parental backgrounds characteristics are additionally controlled for. The coefficient of interest is β_3, presented in columns (3) and (6), which captures the effect of aid eligibility for those who became eligible immediately upon arrival in the destination country.

A broad comparison of results in columns (1) to (3) and columns (4) to (6) shows that the inclusion of parental background characteristics does not change the main pattern in the data. They appear to be important nevertheless, when it comes to the size and statistical significance of the coefficients. We therefore focus on discussing the results shown in columns (4) to (6), given we know that there is selection of aid eligibility on parental characteristics.

Those who arrived in 1953 and thereafter and were eligible for government aid are 16.9 percentage points more likely to have graduated from university. As 98 percent of those who arrived as young adults worked in 1971 (see Table 3.1) eligibility for government aid had no impact on the probability of being employed. However, it significantly increased the chances of working in a good job: those eligible for aid are 18.3 percentage points more likely to work in a high-status job in 1971, and they enjoy on average about 14 percent higher net monthly income (results for the different outcome variables when moving down column 6).

with the worsening conditions in the education sector as well as for certain professional persons such as doctors and lawyers, after 1957 high proportions of refugees were acknowledged as political refugees whose fathers worked in qualified services, culture and religion, or in the public sector (see Figure B.3, Panel C).

Table 3.5: The Impact of Refugee-Specific Government Aid on Educational Attainment and other Socio-Economic Outcomes, Men who arrived in West Germany aged 15 to 25, Cross-Sectional OLS Regression Results (AID specification)

	(1) AID	(2) Arrival aft. 1953	(3) Arrival aft. '53 * AID	(4) AID	(5) Arrival aft. 1953	(6) Arrival aft. '53 * AID
Qualification variables						
High Qualif.	.055	-.195**	.155***	-.012	-.142	.169***
(0/1)	(.046)	(.095)	(.059)	(.040)	(.089)	(.052)
Med Qualif.	-.102*	.315***	-.078	-.047	.237**	-.085
(0/1)	(.052)	(.111)	(.065)	(.052)	(.108)	(.064)
Low Qualif.	.049	-.113	-.075	.059	-.088	-.082
(0/1)	(.044)	(.090)	(.053)	(.045)	(.086)	(.053)
Other Socio-Economic Variables						
Employed (0/1)	-.008	-.033	.020	.001	-.048	.012
	(.016)	(.040)	(.019)	(.015)	(.038)	(.019)
High-status	.070	-.170*	.170***	.011	-.110	.183***
Job (0/1)	(.047)	(.094)	(.061)	(.041)	(.091)	(.056)
Log Net Inc./	.047	-.106	.136***	.003	-.072	.139***
Month (DM)	(.032)	(.075)	(.043)	(.027)	(.069)	(.038)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Parental Controls	No	No	No	Yes	Yes	Yes
N	1,551	1,551	1,551	1,551	1,551	1,551

Notes: Coefficients in columns (1) and (4) show the effect of being eligible for refugee-specific aid on the respective dependent variable. Coefficient in columns (2) and (5) show the effect of having migrated in 1953, or later. Coefficients in columns (3) and (6) show the effect of being eligible for refugee-specific aid and having migrated in 1953, or later. Basic control variables account for age and its square, gender, and year of migration. Parental background characteristics at age 15 capture the father's occupational status (13 categories), the father's industry (16 categories), the father's and the mother's qualification level (6 categories, respectively). Robust standard errors are clustered at the level of the sampling units; there are several hundred clusters. ***, **, and * denote significance at the 1 percent, 5 percent, and 10 percent level, respectively.

So, overall, the refugee-specific government aid had a sizable positive impact on educational outcomes (the average university graduation rate in the group is 21 percent, see Table 3.1) and economic and social integration.

The level effects for AID_i (column 4) are all statistically insignificant, suggesting no general systematic differences between the treatment group and control group once we have controlled for parents' characteristics. This is the effect for the group who arrived before 1953. If anything, potential selection effects should be larger for these early arrivals than for

Table 3.6: The Impact of Refugee-Specific Government Aid on Educational Attainment and other Socio-Economic Outcomes, Men who arrived in West Germany aged 15 to 25, Cross-Sectional OLS Regression Results (EXP specification)

	(1) EXP	(2) Arrival aft. 1953	(3) Arrival aft. '53 * EXP	(4) EXP	(5) Arrival aft. 1953	(6) Arrival aft. '53 * EXP
Qualification variables						
High Qualif.	.003	-.168*	.011***	-.001	-.122	.012***
(0/1)	(.003)	(.094)	(.004)	(.002)	(.088)	(.003)
Med Qualif.	-.006*	.292***	-.007	-.003	.220**	-.006
(0/1)	(.003)	(.111)	(.004)	(.003)	(.107)	(.004)
Low Qualif.	.003	-.116	-.005	.003	-.091	-.005
(0/1)	(.002)	(.089)	(.003)	(.002)	(.086)	(.003)
Other Socio-Economic Variables						
Employed (0/1)	-.000	-.031	.001	.000	-.046	.001
	(.001)	(.040)	(.001)	(.001)	(.038)	(.001)
High-status	.004	-.142	.013***	.001	-.089	.013***
job (0/1)	(.003)	(.093)	(.004)	(.002)	(.089)	(.003)
Log Net Inc./	.003	-.083	.010***	.000	-.056	.010***
Month (DM)	(.002)	(.074)	(.003)	(.001)	(.069)	(.002)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Parental Controls	No	No	No	Yes	Yes	Yes
N	1,551	1,551	1,551	1,551	1,551	1,551

Notes: Coefficients in columns (1) and (4) show the effect of years of exposure (EXP) to governmental aid. Coefficient in columns (2) and (5) show the level effect of having migrated in 1953, or later. Coefficients in columns (3) and (6) show the effect of years of exposure to governmental aid for those who migrated in 1953, or later. Basic control variables account for age and its square, gender, and year of migration. Parental background characteristics at age 15 capture the father's occupational status (13 categories), the father's industry (16 categories), the father's and the mother's qualification level (6 categories, respectively). Robust standard errors are clustered at the level of the sampling units; there are several hundred clusters. ***, **, and * denote significance at the 1 percent, 5 percent, and 10 percent level, respectively.

those who arrived in 1953 and thereafter, because the early arrivals had to apply retroactively for AID_i, sometimes years after they underwent the screening process of the authorities, whereas for the late arrivals the application for AID_i was a natural step at the end of the screening process upon arrival.

We find that those who arrived in 1953 and thereafter are 23.7 percentage points more likely to have a degree from the dual system of apprenticeship which is consistent with the general educational upgrading at the time (column 5).

Table 3.6 shows the results when identification relies on variation in the treatment intensity, that is, the potential years of exposure to aid-eligibility (EXP_i) generated through the differences in age-at-arrival and year of arrival, in combination with the fact that the government program only started in 1953. This allows us to ask whether the effects on the outcome variables varies across treatment levels in the expected direction, and the answer is yes. For those who arrive in 1953, and later, each additional year of potential aid-eligibility increases the probability of graduating from university by 1.2 percentage points, the probability of having a high-status job by 1.3 percentage points, and the net monthly income by about 1 percent.

To put these estimates into perspective, the difference between the 75th percentile and 25th percentile in the potential length of exposure distribution for young adults eligible for aid and who arrived after 1953 is 5 years (see Table 3.3, last row), which translates into a 6 percentage point difference in university graduation as a result of aid-eligibility ($.012 * 5 = .06$), a 6.5 percentage point difference in the probability of holding a high-status job, and a 5 percent higher net monthly income.

Children Sample (Age-at-Arrival: 1-14)

Tables 3.7 and 3.8 show the results of the same specifications and the same outcome variables as those in Table 3.5 and 3.6, but this time for the sample that includes refugees who arrived at ages 1 to 14. At the time of arrival, they were all subject to compulsory schooling, that is, they were naturally integrated in the host country educational institutions. As additional outcome variables, we assess whether in 1971 an individual is enrolled in school or university or participates in apprenticeship training. Basically, none of the coefficients of interest is statistically significant, indicating the refugee-specific aid of this group of refugee kids did not improve educational outcomes or labor market integration.

3.5.2 Robustness

We have dropped respondents who did not report their fathers' occupational status. To assess how this impacts our findings, we replicated our analysis and re-estimated equations (3.1) and (3.3). This time we included all individuals and captured missing occupational status of the fathers by incorporating an additional category in our regression analysis.

In addition, and because the share of AID-eligible refugees increased after 1957, we test the robustness of our results to restricting the sample to the period 1946-1957, instead of 1946-1961 that we use in the main sample. The results for both exercises are similar to those

Table 3.7: The Impact of Refugee-Specific Government Aid on Educational Attainment and other Socio-Economic Outcomes, Men who arrived in West Germany aged 1 to 14, Cross-Sectional OLS Regression Results (AID specification)

	(1) AID	(2) Arrival aft. 1953	(3) Arrival aft. '53 * AID	(4) AID	(5) Arrival aft. 1953	(6) Arrival aft. '53 * AID
Qualification variables						
High Qualif.	-.045	-.066	.025	-.045	-.017	-.008
(0/1)	(.046)	(.065)	(.050)	(.043)	(.065)	(.049)
Med Qualif.	.012	.073	-.001	.024	-.013	.032
(0/1)	(.053)	(.098)	(.065)	(.051)	(.099)	(.064)
Low Qualif.	.033	-.007	-.024	.021	.030	-.024
(0/1)	(.042)	(.089)	(.055)	(.042)	(.092)	(.056)
In School/	.002	-.010	-.072*	.016	.096	.016
University (0/1)	(.026)	(.074)	(.043)	(.025)	(.071)	(.041)
In Apprenticeship	.004	-.038	-.013	.004	-.046	-.004
Training (0/1)	(.010)	(.065)	(.028)	(.012)	(.067)	(.027)
Other Socio-Economic Variables						
Employed (0/1)	-.022	.024	-.045	-.037	-.071	.011
	(.028)	(.075)	(.045)	(.028)	(.071)	(.043)
High-status	.004	-.166***	-.002	.003	-.116*	-.033
Job (0/1)	(.045)	(.063)	(.050)	(.044)	(.066)	(.049)
Log Net Inc./	-.009	-.137	-.052	-.009	-.128	-.074
Month (DM)	(.039)	(.124)	(.063)	(.040)	(.129)	(.064)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Parental Controls	No	No	No	Yes	Yes	Yes
N	1,151	1,151	1,151	1,151	1,151	1,151

Notes: Coefficients in columns (1) and (4) show the effect of being eligible for refugee-specific aid on the respective dependent variable. Coefficient in columns (2) and (5) show the effect of having migrated in 1953, or later. Coefficients in columns (3) and (6) show the effect of being eligible for refugee-specific aid and having migrated in 1953, or later. Basic control variables account for age and its square, gender, and year of migration. Parental background characteristics at age 15 capture the father's occupational status (13 categories), the father's industry (16 categories), the father's and the mother's qualification level (6 categories, respectively). Robust standard errors are clustered at the level of the sampling units; there are several hundred clusters. ***, **, and * denote significance at the 1 percent, 5 percent, and 10 percent level, respectively.

presented earlier.[22]

[22]Results are available from the authors.

Table 3.8: The Impact of Refugee-Specific Government Aid on Educational Attainment and other Socio-Economic Outcomes, Men who arrived in West Germany aged 1 to 14, Cross-Sectional OLS Regression Results (EXP specification)

	(1) EXP	(2) Arrival aft. 1953	(3) Arrival aft. '53 * EXP	(4) EXP	(5) Arrival aft. 1953	(6) Arrival aft. '53 * EXP
Qualification variables						
High Qualif.	-.003	-.070	.001	-.003	-.028	-.001
(0/1)	(.003)	(.064)	(.003)	(.002)	(.064)	(.003)
Med Qualif.	.001	.070	.001	.001	-.006	.003
(0/1)	(.003)	(.096)	(.004)	(.003)	(.097)	(.004)
Low Qualif.	.002	-.001	-.002	.001	.034	-.002
(0/1)	(.002)	(.087)	(.003)	(.002)	(.090)	(.003)
In School/	.000	.011	.004	.001	.107	.000
University (0/1)	(.001)	(.073)	(.003)	(.001)	(.070)	(.003)
In Apprenticeship	.000	-.040	-.001	.000	-.047	-.000
Training (0/1)	(.001)	(.063)	(.002)	(.001)	(.066)	(.002)
Other Socio-Economic Variables						
Employed (0/1)	-.001	.004	-.002	-.002	-.081	.001
	(.002)	(.074)	(.003)	(.002)	(.070)	(.003)
High-status	.000	-.165***	-.000	0 .000	-.122*	-.002
job (0/1)	(.002)	(.062)	(.003)	(.002)	(.066)	(.003)
Log Net Inc./	-.001	-.154	-.002	-.001	-.149	-.004
Month (DM)	(.002)	(.123)	(.004)	(.002)	(.129)	(.004)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Parental Controls	No	No	No	Yes	Yes	Yes
N	1,151	1,151	1,151	1,151	1,151	1,151

Notes: Coefficients in columns (1) and (4) show the effect of years of exposure (EXP) to governmental aid. Coefficient in columns (2) and (5) show the level effect of having migrated in 1953, or later. Coefficients in columns (3) and (6) show the effect of years of exposure to governmental aid for those who migrated in 1953, or later. Basic control variables account for age and its square, gender, and year of migration. Parental background characteristics at age 15 capture the father's occupational status (13 categories), the father's industry (16 categories), the father's and the mother's qualification level (6 categories, respectively). Robust standard errors are clustered at the level of the sampling units; there are several hundred clusters. ***, **, and * denote significance at the 1 percent, 5 percent, and 10 percent level, respectively.

3.5.3 Interpretation

The divergent results for the two samples are noteworthy. Why do we find positive and economically significant effects for the youth sample but reach very different conclusions for the children sample? Upon arrival in West Germany, refugees and their families had few resources at hand. They were also liquidity constrained. That meant refugees arriving as young adults had to consider whether to enter the labor force and immediately earn wages. Our results indicate that refugee-specific aid made a decisive difference. It enabled young adults to postpone their entry into the labor market and, instead, pursue higher education. In the medium-term, this higher education was associated with working in a higher-status job and having higher monthly incomes. These results deserve the attention of policymakers who are addressing refugee issues.

For younger children arriving in West Germany, the economic incentives were quite different. Not only were these refugees too young to start working immediately, but it appears that time spent in the destination country allowed them to catch up with their counterparts who received aid. Presumably, this catch up-process was smoothed by the general expansion of higher education and the exceptional period of economic growth that West Germany experienced. We would like to emphasize that we do not view our results as evidence that aid for young children would be ineffective in general. After all, in the German context that we analyze, all refugees and their children were covered by social security and had access to tuition-free education (Section 3.2). However, the results demonstrate that refugees migrating as children and young adults face different challenges which play an important role in shaping medium-term outcomes.

3.6 Conclusions

This paper examines whether refugees' economic success is linked to government aid. We investigate the impact on refugees who migrated as children (at ages 1 to 14) and on refugees who migrated as young adults (at ages 15 to 24) and assess medium-term outcomes (i.e., at least 10 years after the refugee stream ceased). The analysis focuses on male GDR refugees who migrated to West Germany from the end of World War II until the Berlin Wall was built in 1961. We exploit the fact that West German authorities distinguished between political and economic refugees from the GDR, providing aid only to political refugees. Receipt of this refugee-specific aid, which was meant to compensate for the losses stemming from the refugee experience, did not affect refugees' ability to also receive standard welfare and social security benefits in West Germany. The quasi-experimental nature of this historical setting

allows us to combine several approaches to address identification concerns.

Refugee-specific aid engendered positive and economically meaningful effects for refugees migrating as young adults. For refugees who migrated as children, we find no similar positive effects of the refugee-specific aid on education, employment outcomes, and incomes.

We conclude that age-at-arrival and the institutional link to the host country is important. Refugees migrating as children can catch up with their counterparts who were aid-eligible. This catch-up process, presumably, is related to their natural integration in the host countries education institutions through compulsory schooling laws, the general expansion of higher education and the exceptional period of economic growth that West Germany underwent during the period we study. By contrast, refugees migrating as young adults were more vulnerable to a lack of immediate refugee-specific aid. Faced with the trade-off between entering the labor market and gaining income right away or investing in education, those young adults who were not eligible for refugee-specific aid bypassed investments in education - not surprisingly, given their and their families' severe liquidity constraints. This finding suggests that policymakers need to consider the specific needs of young refugees who are above the compulsory schooling age.

4 A Proposed Data Set for Analyzing the Labor Market Trajectories of East Germans around Reunification

Joint with Dana Müller

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4.1 Introduction

The fall of the Berlin Wall in 1989 and the reunification of Germany in 1990 fundamentally and permanently changed the lives of around 16 Million East Germans (Huinink and Mayer, eds., 1995). These unanticipated events overturned the political, economic, and social systems of the former German Democratic Republic (GDR) at a rapid pace. For East Germans, this resulted in new freedoms and opportunities. At the same time, East Germans had to adapt to the new systemic order and were forced to cope with the economic crisis that was caused by the introduction of the market economy (Akerlof et al., 1991; Burda and Hunt, 2001).

In the GDR, labor market trajectories were remarkably stable up until 1989. Full employment was guaranteed by the state and GDR citizens had the right, as well as the duty, to work (Grünert, 1996; Ritter, 2007). After 1989, prolonged unemployment in East Germany meant that drastic job changes (Diewald et al., 1995, p. 322 et seq.) and regional mobility (Hunt, 2006; Fuchs-Schündeln and Schündeln, 2009) became the norm, rather than the exception.

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Even today, differences between the former East and the former West remain an important dimension of the persistent socio-economic disparities in Germany. Therefore, thorough and transparent scientific investigations of the individual-level labor market consequences of German reunification are still important today. This need for further research has, for example, been documented in a recent study about perceptions concerning the privatization of East German firms after 1989 (Goschler and Böick, 2017).

Administrative data, in particular those stemming from social security notifications and internal procedures of the Federal Employment Agency, are invaluable for analyzing the labor market trajectories of East Germans around reunification. These data are processed at the Institute for Employment Research (IAB) into a biographical dataset, the so-called "Integrated Employment Biographies" (IEB data), which has a panel structure and large sample size. Moreover, these data provide highly reliable information on a number of key variables, such as average daily wages as well as types and durations of labor market episodes.

However, for East Germans data from social security records are only fully available from 1992 onwards. This is due to the fact that the East German labor market administration was integrated into the West German administration, as part of a complex process. It took time before all firms in East Germany started to report to the social security system (Schmid and Oschmiansky, 2007). The resulting gap in the data poses a key empirical challenge. After 1989, significant fractions of East Germans lost their jobs, changed their occupations and industries, or moved to West Germany. A large number of firms closed (e.g., Diewald et al., 1995; Burda and Hunt, 2001; Hunt, 2006). For many research questions, 1992 is thus too late in time as a starting point for analysis.

Our project "Labor Market Trajectories of East Germans around Reunification" partially closes the gap in the data. For this purpose, we obtained the so-called "Data Fund of Societal Work Power" (in German *Datenspeicher Gesellschaftliches Arbeitsvermögen*, which we abbreviate by GAV data) from the Federal Archive of Germany. The GAV data are a cross-section that provides information on labor market relevant characteristics of around 7 million persons for the year of 1989. This amounts to 72 percent of the East German labor force at that time. Based on names, exact dates of birth, and gender, we merged the 1989 data with data from social security records that start in 1992. We thus created a unique and very promising new data set that has two major advantages. First, it allows researchers to study mechanisms behind phenomena of general relevance, such as unemployment, regional mobility, occupational mobility, and mobility across industries. Second, it permits the analysis of East German labor market trajectories around reunification based on a sample size

that is considerably larger than currently existing data sources. From a historical perspective, the new data therefore enhance the analysis of German reunification. From a political perspective, the new data help refine our knowledge of the causes and consequences of the socio-economic disparities between East and West Germany. These disparities constitute a major dimension of inequality that is still extremely relevant in Germany. Note, however, that at this time, the linked data may only be used within the project "Labor Market Trajectories of East Germans around Reunification."

This report is structured as follows. In the next section, we present details on the two original data sources that we merged. In Section 4.3, we describe the merging procedure and evaluate its quality. Section 4.4 concludes and summarizes the next steps.

4.2 Original Data Sources

4.2.1 GAV Data

The so-called "Data Fund of Societal Work Power" constitutes our data source from GDR times. Its German name is *Datenspeicher Gesellschaftliches Arbeitsvermögen*, which we abbreviate by GAV data. "Societal Work Power" is derived from Marxist thought. The authorities in the GDR wanted to refer to a society's combined knowledge, abilities, and skills that are relevant to economic production (Solomon, 1981).

The GAV data were collected in a decentralized way. At the firm and establishment level, human resource departments were required to report information on the characteristics of all employees and had to update these data on a monthly basis. From this source, the councils of each of the fifteen districts of the GDR obtained and combined the information relevant for the GAV data. The councils then transferred this information to the government agency for labor and wages *(Staatssekretariat für Arbeit und Löhne)*, which was ultimately responsible for the collection of the GAV data (Gebauer et al., 2004). The quality of these data meets high standards. In particular, the information reported by establishments was fact checked and had to be revised when implausible (Rathje, 1996), though in a few instances this revision did not take place and thus resulted in missing information (Dietz and Rudolph, 1990).

Neither the original GAV data nor analytical results based on these data were publicly made available. Instead, government agencies in the GDR relied on the GAV data as part of the process of central planning. For example, the data were used to identify and recruit experts demanded in specific circumstances. However, the full potential of the data for central planning purposes was never exploited (Gebauer et al., 2004).

Around 7 million persons are included in the GAV data. Specifically, the data cover the following groups (Dietz and Rudolph, 1990; Rathje, 1996; Gebauer et al., 2004):

- Workers and employees with a permanent or temporary work contract
- Members of producers' cooperative societies *(Produktionsgenossenschaften)* and law firms *(Rechtsanwaltskollegien)*
- Retired persons still working
- Men performing compulsory military service or alternative civilian service

As is typically the case with GDR official statistics, the GAV data exclude the so-called "Sector X," which was an integral part of the GDR regime. For these employees, separate databases existed. Specifically, the following groups are excluded from the GAV data (ibid):

- Persons working for the Ministry of the Interior, the Ministry of State Security, the Socialist Unity Party, the army, or customs authorities ("Sector X")

Separate databases also existed for specific subgroups, such as certain types of teachers and child care workers. Therefore, some groups are only partially included in the GAV data (ibid):

- The data exclude teachers in schools and child care workers; but include teachers at vocational schools, professors at universities, and employees in nurseries.
- The data exclude the self-employed and their employees; though the majority of craftsmen were members of producers' cooperative societies and are therefore included in the data.
- The data include apprentices; but only those who started apprenticeship training in the year before December 1989.
- The data exclude foreigners temporarily working in the GDR under the coverage of intergovernmental agreements; but include foreign GDR residents.

For the workers who are included in the GAV data, rich information was elicited. The variables can be divided into four categories:

1. Demographic characteristics include age, gender, place of residence, the number of children under 14 and the number of persons in need of care in the household, disability status, marital status, and nationality.

2. Qualification characteristics include high school education, current apprenticeship training, and university degree.

3. Employment characteristics include the type of employment, place of employment, leave of absence, main job task, job status, work hours, and occupation.

4. Firm characteristics include firm type and industry.

In our project, we use a cross-section of the GAV data that refers to December 31 in 1989. The history of these data demonstrates that their survival was not self-evident: In fact, the GAV data had been collected on an annual basis.[2] However, due to limited computer capacities in the GDR, only data from the current year were kept while data from previous years were deleted (Gebauer et al., 2004). The Federal Archive of Germany obtained the 1989 GAV data in November of 1991 on magnetic tape. In 1998, the data were for the first time saved on CD-ROM. In addition to the GAV data, detailed wage information had been collected for 2.3 Million employees in the GDR. Unfortunately, because of data protection regulations, these wage data were deleted in 1991 in reunified Germany (Rathje, 1999). The data that we use in our project therefore do not contain any information on pre-unification wages.

The vast majority of historians and other scientists conducting research in the Federal Archive of Germany do not employ quantitative methods (Rathje and Wettengel, 1999). Therefore, up until today, few researchers have analyzed the GAV data. The studies that we are aware of include Solomon (1981) and Groebel (1997). Solomon (1981) provides a technical report concerning the processing and analysis of the GAV data. This report reflects the information technology available in the GDR in the early 1980s.[3] Groebel (1997) explores reasons for the divergence of sectoral employment structures in market economies and planned economies. Among other data sources Groebel (1997) relies on the GAV data to provide descriptive statistics that illustrate her theoretical arguments. Additionally, from 2001 through 2012, a subproject of the Collaborative Research Center 580 studied GDR elites and used the GAV data, though only as a supplementary source of information (Gebauer et al., 2004; Salheiser, 2006).

[2] The GAV data project was initiated in 1975, though it took until 1986 before the data were made fully available to the government agencies interested in them (Gebauer et al., 2004).

[3] Solomon (1981) constitutes a dissertation written at Humboldt-University Berlin (East). During GDR times, this dissertation was classified as confidential. After reunification, a copy was retrieved by the Federal Archive of Germany such that the dissertation can now be accessed by the public.

4.2.2 IEB Data

We merged the GAV data with data from the so-called "Integrated Employment Biographies" (IEB data). The IEB data are a natural choice for our project, because they contain labor market relevant information that resembles the GAV data. The IEB data include information from two sources: social security notifications and internal processes of the Federal Employment Agency. We discuss each of these sources in turn.

First, social security notifications involve an integrated notification procedure for the health, pension, and unemployment insurance programs, which is known by the abbreviation DEÜV (for more details see Wermter and Cramer (1988) and Bender et al. (1996, p. 4 et seq.)). It has been mandatory since 1973 in West Germany and since 1991 in East Germany. The notifications include several pieces of information on all insurable employment episodes reported by every employer. Section 28 of the Social Code Act IV determines what kind of information needs to be notified. In general, a notification includes information about the beginning and end of each employment episode that is subject to social security contributions, as well as corresponding information about gross wages, education, employment status, occupation and nationality. In addition, there is a mandatory notification for every employer liable to social security contributions at least once a year. Since 1999 employment episodes of marginal part-time employees and family workers have also been recorded. Importantly, the social security notifications do not include civil servants, self-employed individuals and regular students.

The following will give a more detailed idea of the notification procedure. The data are recorded by the health insurance companies first, and then are transmitted to the German pension insurance, which in turn forwards the data to the Federal Employment Agency. The data are collected and processed by the Federal Employment Agency, particularly for generating employment statistics. Subsequently the data are processed into employment histories at the Data and IT-Management Department of the IAB. These employment histories constitute the so-called Employee History File, which starts in 1975 for West Germany and includes East Germans from 1992 onwards.

Second, internal processes of the Federal Employment Agency are the other source of the IEB data. These data are collected to fulfill legal requirements, to inform the public and in the preparation of statistics. The data are then prepared at the IAB and organized in four different history files:

1. The Benefit Recipients History includes all periods during which unemployed individuals received earnings replacement benefits from the Federal Employment Agency within the scope of Social Code Book III (SGB III). The data start in 1975.

2. The Unemployment Benefit II Recipient History covers all periods during which unemployed individuals received benefits in accordance with the Social Code Book II (SGB II). It was implemented in 2005 and captures the pooling of unemployment benefits and social assistance. The difference compared with SGB III is that unemployment benefits are not determined individually but depend on the so-called "benefit community" (which includes certain household members, such as spouses and children). This data source only contains information about individuals who are capable of working or are under the age of 64, and about the benefit community's members in accordance with Section 7 of SGB II. However, the Federal Employment Agency is not the only responsible authority for administering the benefits that fall under Social Code Book II. There are three possible types of institutions the data can stem from:

   - Joint facilities of employment agencies and municipalities since 2011 (before 2011 cooperation of employment agencies and municipalities in the context of so-called working partnerships);
   - separated responsibilities until 2011 with divided tasks between the Federal Employment Agency and the municipality; and,
   - authorized municipalities which are responsible for all tasks regarding the SGB II.

   The data originate from different reporting procedures. In particular, authorized municipalities can use their own IT procedures and transmit the data to the Federal Employment Agency. The data have been collected since 2005 but the data are complete only from 2007 onwards.

3. The Participation-in-Measure History Files include active labor market policy measures within the scope of SGB III and in accordance with SGB II if these measures are reported in Federal Employment Agency IT procedures. The data are available from 2000 onwards.

4. The Jobseeker History contains information on jobseekers who are registered with employment agencies. The data are available from 2000 onwards and were expanded in 2005 to also include jobseekers receiving Unemployment Benefit II.

Finally, the data from the social security notifications (i.e., the Employment History File) and the data from the internal processes of the Federal Employment Agency (i.e., the four other history files just described) are combined. Together, these data sources represent the Integrated Employment Biographies (IEB data).

Note that the IEB data could not directly be used for the linkage procedure with the GAV data, because the IEB data lack direct identifiers like names for reasons of data privacy. As we explain in more detail in the next section, we instead used information from the data ware house of the Statistics Department of the Federal Employment Agency. This information includes all individuals from the IEB data as well as their direct identifiers. Variables from the IEB data will then later be merged to the linked new data set.

4.3 Newly Created Data Source

4.3.1 Procedure for the Merge of the GAV Data and the IEB Data

For the purposes of merging the GAV data and the IEB data, the Federal Archive of Germany provided us with the non-anonymized version of the 1989 GAV data. We received fifteen Excel documents, each referring to one of the districts in the GDR, which we transformed into a single file in Stata format. Based on the non-anonymized version of the GAV data, we were able to exploit the following information for the merge: first name, last name, exact date of birth, and gender. In principle, it would have been possible to rely on additional information pertaining to occupations, industries, and regions. However, the IEB data are fully available for East Germans only from 1992 onwards. Between 1989 and 1992, a significant fraction of East Germans changed jobs and moved between regions. Hence, using this additional information would have led to oversampling of persons who did not move across regions or did not change jobs. In order to avoid such biases, we deliberately decided not to use the additional information for the merge and relied on names, date of birth, and gender only.

From the data warehouse of the Statistics Department of the Federal Employment Agency we similarly obtained information on names, date of birth, and gender for persons covered by the IEB data. In addition, we obtained their anonymized personal IDs that will later allow us to merge further IEB variables. Our aim was to identify workers from the GDR and to reduce the complexity of the data. Therefore, when drawing from the universe of individuals included in the IEB data we imposed three restrictions. First, we focused on persons born between 1929 and 1976 who were aged 13 to 70 in 1989. Second, we only included persons for whom at least one episode is recorded in the IEB data between 1990 and 1996 in East or West Germany. Third, we imposed that for these persons no such episodes were recorded in West Germany before 1990. Because of the third criterion, a large number of West Germans

are excluded from the merging procedure. This reduces the likelihood of false matches. At the same time, it implies that we neglect individuals who migrated from West to East Germany before the wall fell. However, only few West Germans moved to the GDR during this period (see for example the graph in Hunt, 2006, p. 1017). Note that, for the individuals who we match, selected variables on their entire history from the IEB records will ultimately be included in the data set.

We conducted the merge in collaboration with Manfred Antoni who describes the technical details of the procedure in Antoni (2018). We began by preprocessing the GAV data. Duplicates in the GAV data were one issue we needed to address (see Table 4.1). On the one hand, this concerns pure duplicates, where all variables are identical to an original observation. We dealt with these cases by dropping all 166,604 pure duplicates. On the other hand, there are cases of data entries that contain information on multiple jobs held by the same individual. Specifically, there are 194,916 data entries which refer to individuals' second or higher order observations (Table 4.1). We do not know with certainty whether these are observations referring to individuals performing several jobs in parallel or observations referring to individuals' previous jobs. The latter case would refer to situations in which the data were not updated after job changes. We were, however, able to code a variable ordering multiple data entries per person by the date the data were collected. One possibility is therefore to restrict the analysis to each individual's most recent job spell, which we did when merging.[4] This left us with a sample size of more than 7 million persons included in the GAV data (Table 4.1).

We next preprocessed the information on names. In the original GAV data, information on names is presented in the format of "*last_name, first_name*". While this is true for the majority of names, we had to account for the fact that some names deviated from the intended format. To ensure comparability across data sources, identical preprocessing steps were applied to both the GAV data and the IEB data. In particular, the following steps were necessary:

- Special characters, which appeared in various formats and at varying places in the name variable, were deleted or replaced by a comma to separate first and last names (for example when the name was in the format of "*„last_name, first_name*" or "*last_name. first_name*" etc.).

- Name suffixes, which appeared in various formats and at varying places in the name

[4] When we performed the merge, we also encountered the issue of duplicates in the IEB data. These are cases where we found matches based on exact names and date of birth but where we could not identify a unique IEB person ID. However, this concerns few cases (<1%) which we treat as unsuccessful matches.

Table 4.1: Number of Observations and Persons in the GAV Data and Merging Quotas

Original number of observations in GAV data	7,412,001
*Among these: Number of pure duplicates**	166.604
Actual number of observations in GAV data	7,245,397
*Among these: Second or higher order observations for persons in the GAV data***	194,916
Number of persons in GAV data	7,050,481
Among these: Persons with name of four letters or less	16,406
Number of persons in GAV merge was based on	6,978,591
Among these: Last name and first name available	6,479,700
Among these: Last name available only or first name and last name not separated by a comma	498,883
Number of GAV persons identified in IEB data	5,407,817
Percentage of persons in GAV data for whom a match was found	
All	0.7670
Women Only	0.7240
Men Only	0.8048
Younger than 60, all	0.8221
Younger than 60, women only	0.7680
Younger than 60, men only	0.8706

Notes: * indicates cases where all variables were identical; ** refers to cases of second or higher order entries for a person with the identical name, date of birth, and gender.

variable, were deleted. This was relevant for academic titles (such as "Dr." or "Professor"), titles of the nobility (such as "Von", "Graf" or "Freiherrin"), and generational designations (such as "Junior" or "Sr. "). It also concerned farmers with a supplementary last name (such as "*last_name1 genannt last_name2*" where the suffix "genannt" was deleted).

In the GAV data, additional peculiarities in the name variable required further investigation. We therefore made the following adjustments:

- There are cases in the GAV data where the information for "*last_name, first_name*" consists of less than four letters. Often, these letters do not constitute plausible names. In some cases, these letters refer to actual, short last names, where no information on

the first name is provided. According to experience from previous data record linkages performed at the Research Data Center of the Federal Employment Agency, it is highly unlikely to find matches in the IEB data based on name information of four letters or less. Therefore, we decided to exclude these cases from the merge. This concerned around 16,000 persons (see Table 4.1).

- In the GAV data, for around 500,000 persons, the information "*last_name, first_name*" consists of one word only. In the majority of cases, one-word-names refer to last names. We therefore interpreted these names as last names and used this information for the merge. Additionally, there are one-word-names including a last name and a first name which lack a separating comma. In order to distinguish between first and last names, we used a routine that identified and separated common first names. We then fact checked these results manually, since additional corrections were required that could not be automated (for example when a common first name was in fact part of a last name as in "Franke" , "Schubert" or in more exotic semantic combinations).

After preprocessing, we based the merge on 6.98 million persons included in the GAV data. For 93 percent of these, we used information of first and last names, whereas for the remaining 7 percent either the last name was available only or first and last names were not separated (Table 4.1). To put these figures into perspective, the GDR labor force of 1989 consisted of 9.75 million persons (Federal Statistical Office, 1994). Thus, our merge encompasses 72 percent of the East German labor force.

4.3.2 Evaluation of the Merging Procedure

For 77 percent of persons from the GAV data we found a match in the IEB data (Table 4.1). According to experience from previous merges performed at the Research Data Center of the Federal Employment Agency, this is a good quota. For the vast majority of matches (88 percent), the information on first and last names, date of birth, and gender was identical in both the GAV and the IEB data (data not shown). The remaining fraction was matched using record linkage techniques that tolerate a justifiable degree of error while at the same time keeping the likelihood of false matches as small as possible. Three steps were particularly relevant in increasing the merge quota. First, we tolerated small spelling or coding mistakes in the name information and in the day of birth, but imposed that the other identifiers (gender, month and year of birth) were matched accurately. Second, we required a perfect match between last names, birth date, and gender but dropped first names. This step was especially important; as a significant fraction of persons in the GAV data lack a first name (see Table

4.1). Third, we repeated the previous step but relied on first names while neglecting last names. We only kept cases where a unique match was found. Manfred Antoni performed these steps and provides more details on the exact implementation in Antoni (2018).

We next use OLS-regression analysis to investigate how the success of the merge correlates with key observable characteristics in 1989. Specifically, we regress a dummy variable that is equal to one in case of a successful merge on key observable characteristics. Key observable characteristics are measured as categorical variables and refer to gender, age intervals, type of school diploma obtained, and marital status. The regression results are displayed in Table 4.2. For reference, we display corresponding summary statistics for the independent variables in Table 4.3.

Based on column (1) in Table 4.2, three phenomena should be emphasized. First, the merge quota is considerably lower for individuals older than 60 in 1989. Indeed, we were not able to find any matches above the age threshold of 61 (data not shown). This is due to the fact that for the IEB data the information on names stems from the late 1990s. By this time, older East Germans had dropped out of employment. Most analyses based on the new data set should therefore be limited to persons younger than 60. If these are dropped, the quota of successful merges increases to 82 percent (see Table 4.1). Below the age threshold of 50 in 1989, the same quota increases further to 86 percent (data not shown). In our view, these are high merge quotas that speak for the quality of the new data set. At the same time, these quotas highlight that the data are more reliable for persons younger than 60 or even 50 in 1989.

Second and reassuringly, there are only negligible differences by qualification level in the success of the merge. This can be seen in column (1) of Table 4.2, where we measure qualification in terms of four different levels of school diploma obtained. We used these categories because the usual distinction between an apprenticeship degree and no formal vocational qualification cannot be made in the GAV data. The only noteworthy difference by qualification level is the significantly lower merge quota for persons with missing information about the school diploma obtained. However, this concerns very few cases (< 0.5 percent, see Table 4.3).

Third, the merge quota is considerably lower for women than for men. For women younger than 60 in 1989, this quota amounts to 77 percent, which is around 10 percentage points lower than the merge quota of their male counterparts (see Table 4.1). We investigate this further in columns (2) and (3) of Table 4.2, where we perform regression analysis as before but this time split the sample by gender. Furthermore, we exclude persons older than 59.

We show that the likelihood of finding a match among women is lowest for initially single

Table 4.2: OLS Regression Results Assessing the Success of the Merge by Age, Qualification Level, and Marital Status

	(1) All	(2) Women, < 60 years	(3) Men, < 60 years
Female	-.1003***		
	(.0003)		
Age intervals			
$\geq$ 20 & < 30 years	.0047***	.0064***	-.0121***
	(.0008)	(.0014)	(.0008)
$\geq$ 30 & < 40 years	.0351***	.0744***	-.0128***
	(.0008)	(.0014)	(.0009)
$\geq$ 40 & < 50 years	.0371***	.0975***	-.0253***
	(.0009)	(.0015)	(.0010)
$\geq$ 50 & < 60 years	-.1557***	-.2111***	-.1112***
	(.0009)	(.0016)	(.0010)
$\geq$ 60 years	-.8137***		
	(.0009)		
School Diploma			
8 years of schooling	-.0101***	-.0189***	.0009
	(.0006)	(.0011)	(.0007)
10 years of schooling	.0014**	.0001	.0041***
	(.0006)	(.0011)	(.0007)
Abitur (12 years)	-.0237***	-.0211***	-.0208***
	(.0007)	(.0014)	(.0009)
Missing	-.1166***	-.1009***	-.1695***
	(.0137)	(.0217)	(.0228)
Marital Status			
Married	.0846***	.1650***	.0269***
	(.0004)	(.0008)	(.0005)
Widowed	.0114***	.0399***	-.0290***
	(.0010)	(.0019)	(.0028)
Divorced	-.0111***	.0122***	-.0175***
	(.0007)	(.0011)	(.0009)
Missing	.0283**	.0837***	-.0100
	(.0143)	(.0224)	(.0238)
Constant	.8355***	.6659***	.8914***
	(.0009)	(.0016)	(.0009)
R squared	.2753	.1077	.0153
N	7,050,410	3,102,800	3,453,027

Notes: OLS-regression analysis of a dummy variable equal to 1 in case we were able to find an individual included in the GAV data also in the IEB data and equal to 0 otherwise on key observable characteristics measured in 1989 (gender, age intervals, type of high school diploma obtained, and marital status). Reference categories are: male (column (1) only), age below 20, no high school diploma indicating at least eight years of schooling, and being married. The sample includes all individuals from the GAV data, but excludes observations when the same individual is included in the GAV data more than once (see Table 4.1). In column (1), very few individuals are excluded from the regression due to missing age information. Robust standard errors are in parenthesis. *** and ** denote significance at the 1 and 5 percent levels.

Table 4.3: Summary Statistics (in Percent)

	(1) All	(2) Women, < 60 years	(3) Men, < 60 years
Female	46.73		
Age intervals			
< 20 years	4.58	4.81	5.02
$\geq$ 20 & < 30 years	23.29	25.95	24.24
$\geq$ 30 & < 40 years	24.77	26.32	26.92
$\geq$ 40 & < 50 years	19.67	21.04	21.25
$\geq$ 50 & < 60 years	20.68	21.88	22.57
$\geq$ 60 years	7.01		
Missing (excluded from regressions)	0.00		
School Diploma			
< 8 years of schooling	6.84	4.92	9.10
8 years of schooling	40.74	37.22	37.55
10 years of schooling	44.38	50.28	44.26
Abitur (12 years)	7.84	7.31	8.94
Missing	0.20	0.26	0.16
Marital Status			
Single	22.34	19.50	27.46
Married	67.18	68.46	64.92
Widowed	2.35	2.46	0.64
Divorced	7.95	9.33	6.83
Missing	0.19	0.25	0.14
N	7,050,481	3,102,800	3,453,027

Notes: Summary statistics for independent variables used in the repression analysis presented in Table 4.2. All variables refer to 1989.

women. In particular, the merge quota is considerably higher for those who are married in 1989 compared with those who are single initially.[5] This indicates that the lower merge quota among women can be rationalized by the fact that initially single women changed their names after marriage and are then more difficult to identify in the IEB data.[6] Our

[5] With 68 and 20 percent, respectively, initially married and initially single women are the largest group; 9 percent of women are divorced, and 2 percent are widowed (Table 4.3, column (2)).

[6] In general, when East German women married for the first time, they usually adopted the husband's family name, but kept this name after a divorce. Thus, for our merge we are particularly concerned about women's transitions from initially being single to being married, while we are less concerned about transitions from initial marriages into divorce. Our reasoning is based on anecdotal evidence from discussions with former employees of the former population registration office in Karl-Marx-Stadt (now Chemnitz). We are not aware of systematic

recommendation is therefore that future analyses should include robustness tests concerning women's initial marital status. It might for example be appropriate to add marital status as a control variable or to investigate whether main effects differ by marital status category.

4.4 Conclusions and Next Steps

This report summarizes our linkage of the GDR's "Data Fund of Societal Work Power" from 1989 with the Integrated Employment Biographies from later years. The merge was based on around 7 million East German workers, which amounts to 72 percent of the East German labor force at that time. We were able to obtain a comparatively high merge quota of 82 percent among persons younger than 60 in 1989. However, the merge was somewhat less successful for older workers, who dropped out of the labor force, and for initially single women, who often changed their names after marriage.

Before the linked data can be used in the project "Labor Market Trajectories of East Germans around Reunification," different processing steps are still necessary. First of all, the direct identifiers such as names have to be deleted and will be replaced by a pseudo-identifier. Variables from the IEB, which are normally contained in the linked standard data products of the Research Data Center (FDZ), such as PASS-ADIAB (see Antoni and Bethmann, 2018), will be added to the new data set. In addition, we need to develop crosswalks in order to harmonize variables that are similar in the GAV and IEB data, such as the occupation, industry, and region variables. Finally, a sample needs to be conceptualized and drawn from the linked data.

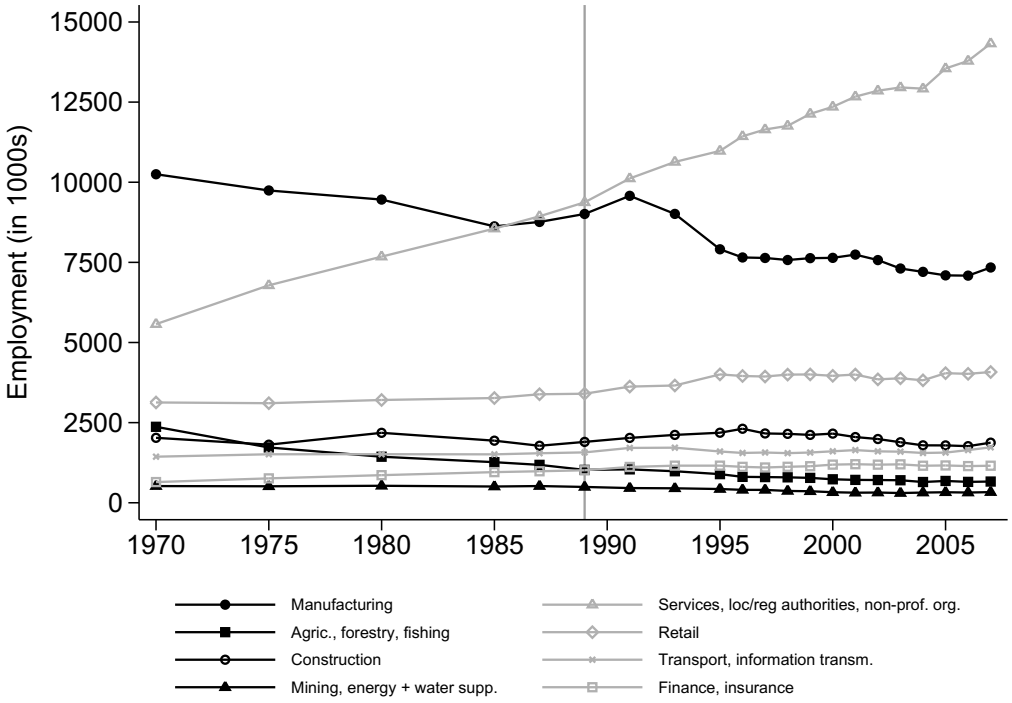
The linked data will allow the analysis of research questions about East German employment biographies around reunification based on sample sizes that are much larger than those of currently existing data sources with panel structure. However, our legally binding agreement with the Federal Archive of Germany requires that the data will have to be deleted after the end of our project. Therefore, the Research Data Center (FDZ) of the Federal Employment Agency will develop a concept in order to convince the Federal Archive to make the linked data available to the research community via the FDZ. We believe that the new data have considerable potential to answer research questions of general scientific interest, as well as to enhance the understanding of differences between East and West Germany, which still constitute a major dimension of the persistent socio-economic disparities in this country.

studies that analyze norms concerning female names for East Germany.

A Appendix to Chapter 2: The Impact of a Negative Labor Demand Shock on Fertility

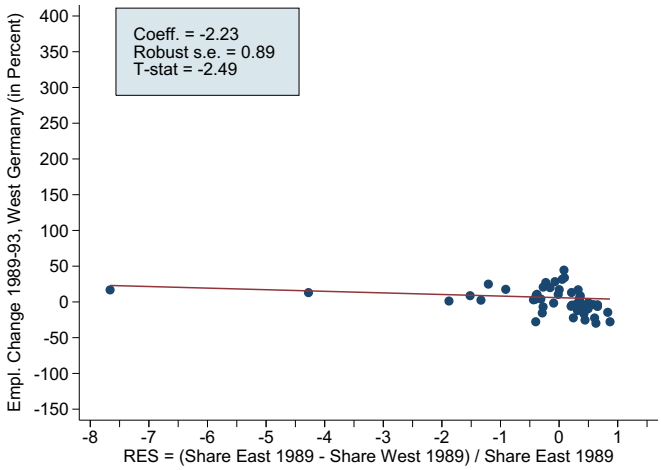
A.1 Employment by Sector and More Detailed Industries, Additional Figures

Figure A.1: Employment in West Germany (in Thousands), By Economic Sector, 1970-2007 (Selected Years)



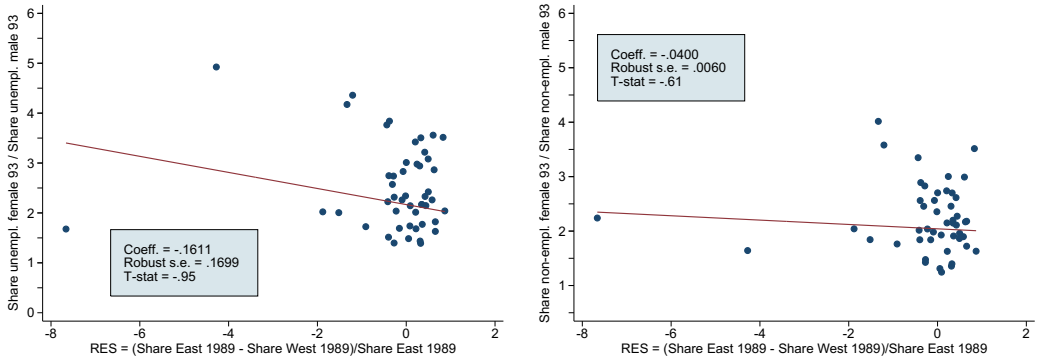
Source: *1970, 1975, 1980:* Provided by Federal Statistical Office and based on the (West German) Microcensus. *1985 and later:* Author's calculations based on the Scientific Use Files of the Microcensus (a 0.7 percent sample of the population) for persons aged 15 and older living in West Germany at their main residence; weighted by Microcensus weights. The increase and decline in manufacturing employment between 1989 and 1995 can be explained by new investment opportunities in East Germany, followed by the impact of a now more competitive environment in which firms could move production to central and eastern European countries (see, e.g., Dustmann et al., 2014, p. 182).

Figure A.2: Correlation between the Relative West German Employment Change 1989 to 1993 (in Percent) and Relative Excess Supply in 1989, By Industry



Source: West German analogue for Figure 2.4, panel (A). $N = 48$, the regression model is weighted using 1989 West German employment shares of industries as analytical weights. The y-axis displays change in percent, i.e., $100 * \frac{Empl\_West_{j,93} - Empl\_West_{j,89}}{Empl\_West_{j,89}}$.

Figure A.3: Female versus Male Unemployment and Non-Employment Rates in East Germany 1993 and Relative Excess Supply in 1989, by Industry



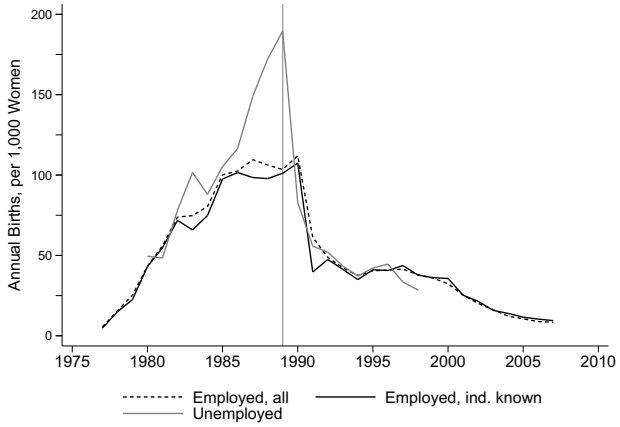
A. Female versus Male Unemployment Rates in 1993

B. Female versus Male Non-employment Rates in 1993

Source: The analysis is based on the Scientific Use File of German Microcensus of 1993 and on persons aged 15 to 54 living in East Germany at their main residence. On the y-axes, I display female unemployment (non-employment) rates divided by male unemployment (non-employment) rates by industry. These ratios are regressed on the RES measure. The regression model is weighted using the 1993 shares of the industries as weights.

A.2 Initially Unemployed Women

Figure A.5: Annual Number of Births per 1,000 East German Women, By Employment Status at the Beginning of 1991



Notes: Group definitions and sample sizes are: (1) 9,017 women were employed on January 1st, 1991 ("employed, all"). (2) Of those employed on January 1st, 1991, 4,234 women had non-missing industry information ("employed, industry known"). This is the main sample used in the analysis. (3) An additional 807 women were unemployed on January 1st, 1991 ("unemployed"). The discrepancy between the first two groups motivates the use of weights in order to make the sample representative by industry; whereas weighting cannot address any selective attrition resulting from unemployment. Note also that 2 percent of women had already migrated to West Germany by January 1st, 1991. Unfortunately, due to its small sample size (N=223), I was not allowed to display birth data for this group. This is because results based on less than 20 absolute births per year and subgroup were censored. For the same reason, the line for initially unemployed women is for years 1980 to 1998 only.

Eight percent of women had already become unemployed by January 1st, 1991, the day for which the sample was drawn. Across and within industries, these women should be among those workers with the highest risk of job loss. Since their initial industry is not known, it is unfortunately not possible to include them in the main analysis. Nevertheless, interesting fertility patterns are found for this subgroup.

To illustrate this further, in Figure A.5, annual births per 1,000 women are displayed for women who work at the beginning of 1991 as well as for women who have already lost their jobs by then. It turns out that the initially unemployed women have disproportionately high birth rates in the three years prior to reunification (1987 to 1989). Those East German women who were among the first losing their jobs were relatively likely to have young children below the age of four. This corresponds to legal regulations: The comprehensive job guarantee for all workers in the GDR was officially abolished in June of 1990 (Richardi, 2007, p. 356); whereas special dismissal protection and job guarantees for pregnant women and for women on maternity leave (which originate from the West German labor law) were introduced in East Germany only at the beginning of 1991 (ibid, p. 359). Overall, the association in Figure A.5 suggests that in these times of elevated economic uncertainty, childbearing was related

Table A.1: Fertility Statistics, By Employment Status and Non-Missing vs. Missing Industry Information on January 1st, 1991

	(1) Children born bf. 1991 (#)	(2) Children born 1991-2007 (#)	N
(a) Main sample: employed, industry known (Jan. 1st, 1991)	0.952	0.522	4,234
(b) Employed, industry not known (Jan. 1st, 1991)	0.965	0.531	4,066
(c) Employed, industry not known OR on leave (Jan. 1st, 1991)	1.056	0.534	4,783
(d) Unemployed (Jan. 1st, 1991)	1.159	0.560	807

Notes: Means of the number of children born before 1991 (column (1)) and between 1991 and 2007 (column (2)). Note that the group referred to in panel (b) is a sub-group of the group referred to in panel (c).

to the risk of job loss.[1]

Table A.1 presents further fertility statistics for women with missing and non-missing industry information. Fertility statistics for the main sample (panel a) compare well to statistics for women who on January 1st, 1991, were employed but for whom no industry information is known (panel b). By construction, the mean of the number of children born before 1991 increases once women are included who on January 1st, 1991, were on maternity or sick leave (panel c).[2] Finally, as has been discussed before, women who had already lost their jobs by January 1st, 1991, up until 1991 had more children on average than the other groups of women (panel d).

A.3 Women Employed in Social Security Agencies

My analysis excludes women initially employed in social security agencies. Social security agencies practically did not exist in the GDR. Employment in this industry therefore increased by more than 850 percent between 1989 and 1993. The corresponding RES value is similarly extreme. Despite this strong employment growth, women initially employed in social security agencies were frequently replaced by new workers. This was part of the complete reorganization of social security agencies after the reunification of Germany (Bernien et al.,

[1] From 1991 onwards, dismissal protection for mothers only covered the time of maternity leave; it moreover did not apply to women employed in firms which closed down.

[2] In this context, it is a concern that some women referred to in panel (c) became pregnant after the wall fell and gave birth before 1991; such that these births are not captured by my analysis. However, this is not the case for 94 percent of women referred to in panel (c) or for 97 percent of women referred to in panels (a) and (c) combined.

Table A.2: Extreme Cases: Social Security Agencies Compared With Insurance and Financial Intermediation, Various Outcomes, OLS Estimates (Panel Regressions, Unweighted)

	(1)	(2)	(3)	(4)	(5)	(6)
	1991-94		1995-99		2000-07	
(a) Dep. Var.: Unemployment Spell in t (0/1)						
Soc. Sec. Agencies	.0211	.0219	.0817***	.0515	.0430*	.0214
	(.0321)	(.0371)	(.0291)	(.0322)	(.0222)	(.0243)
(b) Dep. Var.: Industry Change in t (0/1)						
Soc. Sec. Agencies	.1468***	.1196***	.0333	.0421*	.0273**	.0232*
	(.0294)	(.0321)	(.0205)	(.0242)	(.0107)	(.0125)
(c) Dep. Var.: Birth in t (0/1)						
Soc. Sec. Agencies	.0210	.0064	.0286***	.0299***	-.0035	-.0006
	(.0140)	(.0178)	(.0101)	(.0093)	(.0081)	(.0076)
Age controls only	yes	-	yes	-	yes	-
Main controls	-	yes	-	yes	-	yes
Time FEs	yes	yes	yes	yes	yes	yes
N (each year)	228	228	228	228	228	228

Notes: Each coefficient is from a separate regression. It refers to a dummy variable equal to one for women who worked in social security agencies on January 1st, 1991; this dummy variable is equal to zero for women who worked in insurance or financial intermediation on January 1st, 1991. Control variables are analogous to Table 2.2. Robust standard errors are in parentheses; ***, **, * refers to significance at the 1, 5, and 10 percent level, respectively. No weights were used.

1996).

Once I include women initially employed in social security agencies, the economic significance of the RES demand shock decreases somewhat. To illustrate this, in Table A.2 I compare outcomes of women initially employed in social security agencies with outcomes of women initially employed in insurance and monetary intermediation industries. These are two other examples of industries which expanded strongly after reunification, although less so than social security agencies. Table A.2 reveals that women initially employed in social security agencies comparatively often experienced unemployment and, in particular, changed industries often. Their medium-term birth rates were relatively high. In this sense, for women initially employed in social security agencies, less favorable or stable labor market outcomes are again associated with higher medium-term birth rates; but, this stands in contrast to the exceptional employment development of their initial industry.

A.4 Description of Variables

Table A.3: Variables and Underlying Concepts

Unemployment:
Because unemployment transfers are in part means tested (Fitzenberger and Wilke, 2004), unemployment refers to spells with such transfers (unemployment benefits or assistance) or to spells merely entailing contributions towards pensions. Unemployment variables are defined for calendar years; unemployment spells covering multiple calendar years were thus split.

Industry Change:
Defined as occurring whenever a woman works in a new industry for the first time.

Migration West:
Based on a Stata-routine by Dana Müller. East or West German residence is inferred from employment and unemployment spells and related pension entitlements. There may be gaps in the data; thus, the point of time of migration is determined as the last day of the last spell in East Germany.

Birth: Inferred from month of birth of a woman's children.

Age: Inferred from a woman's month of birth.

Qualification: Imputed and defined following Fitzenberger et al. (2006).

Children Born Before 1991:
Inferred from month of birth of a woman's children.

Large/very large city:
Cities are based on the 2009 regional classification scheme; in several cases the definition of city boundaries is broader than at the beginning of the 1990s. Moreover based on the first entry when regional information was non-missing. Large cities are defined as Chemnitz, Halle, Magdeburg, Erfurt and Rostock; very large cities are Berlin, Leipzig, and Dresden.

GDR regime closeness:
Proxied by GDR pension privileges. Around one third of these pensions were paid to "Sector X" employees and two-thirds to persons in positions considered important, including part of the intellectual elite and pedagogues (Schmähl, 2007).

Apprenticeship Training:
Dummy variable equal to one for women who on January 1st, 1991, participated in an apprenticeship training program.

Table A.4: List of Industries in Ascending Order of RES

Industries j=1,...,48:
Insurance; Monetary intermediation; Printing, reproduction; Finishing trade; Construction & repair of road vehicles etc.; Drawing & cold-rolling of metals, custom steel forming; Accommodation, homes, laundry, cleaning, waste collection etc.; Wholesale, wholesale on a fee or contract basis; Chemical industry, manufacturing & processing of fissible & fertile material; Manuf. of iron, sheet metal & metal products; Retail sale; Manuf. of plastics products, processing of rubber products; German Federal Postal Administration; Precision mechanics, optics, Manuf. of watches & clocks; Local & regional authorities; Human health activities, veterinary activities; Other services (consulting & related activities); Other transport; Manuf. & processing of pulp, groundwood pulp, paper, paperboards; Iron & steel producing industry; Manufacturing & processing of wood; Facture of structural metal products, Manuf. of railway vehicles; Non-profit organizations; Manuf. of non-ferrous & semi-finished non-ferrous metal products; Manuf. of tobacco products; Electrical engineering, repair of domestic appliances; Main construction; Manuf. of food products; Fine ceramics, Manuf. & processing of glass; Mechanical engineering; Inland, sea & coastal water transport; Education, science, culture, publishing; Manuf. of wearing apparel; Manuf. of beverages; Distribution of electricity, gas, water, long-distance heating & related activities; Industrial market gardening & farming, forestry, fishing; Quarrying & processing of stone & minerals; Foundry; Coal mining; Manuf. of office machinery, accounting & computing machinery; Manuf. of musical instruments, games, toys, fountain pens etc.; Manuf. of textiles; Manuf. of leather; Building & repairing of ships; Agriculture; Railway organizations; Manuf. of refined petroleum products; Other mining

The industry classification scheme corresponds to the German 1979 Classification of Economic Activities.

Table A.5: RES and Total Number of Births/End of Period Childlessness, OLS Estimates (Cross-Sectional Regressions), By Migration Status

	Non-Migrants			Migrants		
	(1)	(2)	(3)	(4)	(5)	(6)
	1994	1999	2007	1994	1999	2007
(a) Dep. Var: End of Period Childlessness (0/1)						
RES	-.0166	-.0467***	-.0379**	-.0329*	-.0304	.0442**
	(.0165)	(.0109)	(.0151)	(.0182)	(.0249)	(.0202)
P90 vs P10	-.0247	-.0695	-.0564	-.0490	-.0454	.0658
N	1,043	1,043	1,043	554	554	554
(b) Dep. Var: Total Number of Births (#)						
RES	.0124*	.0469***	.0426***	.0137	.0217**	.0046
	(.0070)	(.0057)	(.0107)	(.0082)	(.0096)	(.0165)
P90 vs P10	.0184	.0699	.0634	.0204	.0323	.0069
N	3,036	3,036	3,036	1,198	1,198	1,198
Main controls	yes	yes	yes	yes	yes	yes

Notes: As in Table 2.4. A woman is defined as being a "migrant" if she migrated to West Germany before 2007; all other women are "non-migrants."

A.5 Robustness of the Composition Effect

In Section 2.6 in the main text, I have established that the RES demand shock altered the composition of mothers, since women more severely affected by the shock had relatively more children than their counterparts who were less severely affected. Here I present and discuss the robustness analysis that I merely summarized in the main text in Section 2.6.3.

A.5.1 Migration to West Germany

To begin with, it is of interest how the results are impacted by migration to West Germany, which played an important role for the cohorts studied. Thus, I split the sample between those who stayed in East Germany and those who migrated to West Germany at some point before 2007 (Table A.5). The positive effect of the RES demand shock on births during the 1990s is confirmed for migrants and non-migrants. Yet the effects are stronger among non-migrants and persist in the long term only for this group. This is not surprising, as migration to West Germany was likely associated with new economic opportunities. For migrants it may have become less relevant which industries they were employed in initially. Nevertheless, to comprehensively assess the effects of the demand shock, I include migrants

Table A.6: RES and Qualification in 1991, OLS Estimates (Cross-Sectional Regressions)

	(1) Low qualif. (0/1)	(2) Med. qualif. (0/1)	(3) High qualif. (0/1)
RES	-.0051	.0014	.0038
	(.0155)	(.0200)	(.0054)
P90 vs P10	-.0076	.0020	.0056
Mean of dep. var.	.255	.706	.039
Main Controls	yes	yes	yes
N	4,234	4,234	4,234

Notes: Each coefficient is from a separate regression. The dependent variables are dummy variables capturing no formal qualification (column 1), completed apprenticeship training (column 2), and university graduation (column 3). They all refer to the beginning of 1991. Standard errors and control variables are defined as before (see Table 2.4), but control variables exclude qualification variables.

throughout the analysis.

A.5.2 Placebo Test for Education

Previous research has shown that low-skilled East German women disproportionately lost their jobs after reunification (Hunt, 2002). In this context, the skill-level of the employed in an industry could be a characteristic which might have influenced the RES-measure and which could also be related to fertility rates. While I control for education throughout the analysis, I additionally conduct a placebo test where I regress women's initial qualification levels in 1991 on the RES-measure (see Table A.6). Reassuringly, it turns out that there is no systematic association between the RES-measure and qualification levels.

A.5.3 Child Care and Regional Spillover Effects

Next, I investigate whether the results are driven by regional spillover effects. The availability of childcare could be relevant in this context. The GDR was a state incentivizing and even requiring full-time employment of mothers through comprehensive public child care (Rosenfeld et al., 2004). Post-reunification, public child care became the responsibility of local municipalities. Its availability declined in East Germany, even though it was still markedly higher than in West Germany (Kreyenfeld, 2003, p. 310). However, in municipalities more severely impacted by the demand shock, one would expect the availability of child care to be relatively lower. This should decrease regional birth rates and lead to underestimation of the effect of the RES demand shock on the composition of mothers. While the reduced availability of child care is one plausible reason for the East German fertility decline as a

Table A.7: RES and Total Number of Births/End of Period Childlessness, OLS Estimates (Cross-Sectional Regressions), Controlling for Regional Spillover Effects

	(1) 1994	(2) 1999	(3) 2007	N
(a) Dep. Var: End of Period Childlessness (0/1)				
RES	-.0189*	-.0417***	-.0179*	1,516
	(.0110)	(.0093)	(.0090)	
P90 vs P10	-.0281	-.0621	-.0267	
(b) Dep. Var: Total Number of Births (#)				
	.0107***	.0373***	.0286***	4,088
	(.0031)	(.0040)	(.0084)	
P90 vs P10	.0159	.0555	.0425	
Main Controls	yes	yes	yes	
Municipality FEs	yes	yes	yes	

Notes: As in Table 2.4. The regressions now contain detailed fixed effects accounting for the municipality a woman lived in at the beginning of the 1990s (*Kreise* in German). In a few cases, this information was missing, which explains the slightly smaller sample sizes.

whole; it is less plausible as a cause for the finding that women more severely impacted by the demand shock had more children on average.

Independent of child care, it could still be the case that the RES demand shock impacted fertility through the channel of regional spillover effects. Such spillover effects would be a consequence of the labor demand shock, but one that would indirectly operate through differential changes in regional economic prospects. To assess whether this channel impacts the results, in Table A.7 I include detailed fixed effects accounting for the municipalities women lived in at the beginning of the 1990s (*Kreise* in German). The inclusion of these fixed effects does not change the results significantly. Point estimates for results on childlessness are very similar to those presented earlier, and point estimates for the total number of children born become only slightly smaller. This suggests that the channel of regional spillover effects played only a small role in explaining the results.

A.5.4 Missing Information on Industry

My sample includes women whose firms were integrated into the social security system and reported their industry affiliation already at the beginning of 1991. A potential concern is that firm-level characteristics, such as firm size, determined which firms were more likely to report their industry affiliation earlier. I therefore analyze whether firm-level characteristics confound the results. In Table A.8, I control for the set of 1991 firm-level characteristics

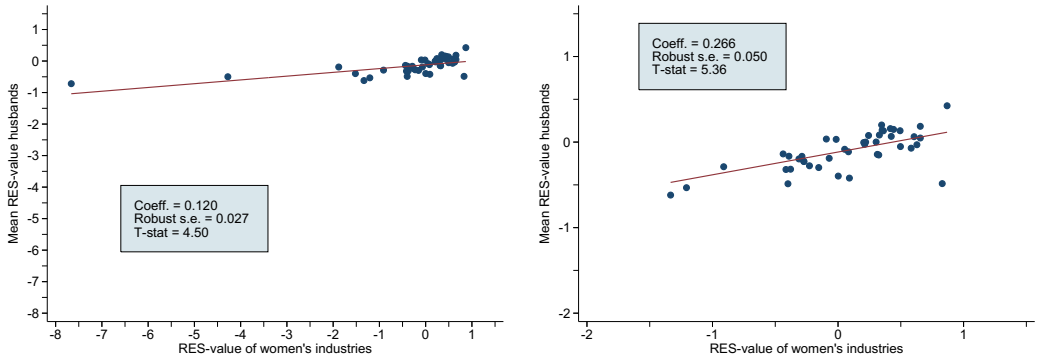
Table A.8: RES and Total Number of Births/End of Period Childlessness, OLS Estimates (Cross-Sectional Regressions), Including Firm-Level Control Variables

	Dep. Var: End of Period Childlessness (0/1)			*Dep. Var: Total Number of Births (#)*		
	(1)	(2)	(3)	(4)	(5)	(6)
	1994	1999	2007	1994	1999	2007
RES	-.0275**	-.0458***	-.0204*	.0158***	.0407***	.0367***
	(.0118)	(.0103)	(.0105)	(.0038)	(.0045)	(.0098)
P90 vs P10	-.0410	-.0682	-.0304	.0235	.0606	.0547
Firm-Level Control Variables						
Share Female	-.1661**	-.2000***	-.2006***	.0996***	.1296***	.2062***
	(.0724)	(.0648)	(.0563)	(.0287)	(.0303)	(.0545)
Share low-skilled	-.0899**	-.1402*	-.1043*	.0380**	.0641	.1181**
$>$ 5% (0/1)	(.0389)	(.0755)	(.0532)	(.0184)	(.0473)	(.0581)
Share younger 30	.1659	-.0007	-.1525	-.0068	.0222	.2087*
	(.1130)	(.0871)	(.1177)	(.0701)	(.0753)	(.1214)
Share older 50	-.0591	-.2135	-.2843	.1357	.2480	.3373*
	(.1659)	(.1862)	(.1180)	(.0905)	(.1707)	(.1826)
Firm Size	1.06E-05	4.89E-06	5.33E-06	-4.97E-06	-7.74E-06	-1.2E-05
	(8.80E-06)	(1.24E-05)	(9.27E-06)	(5.97E-06)	(7.75E-06)	(9.79E-06)
Firm Size Sq.	-3.45E-10	-2.87E-11	-4.80E-11	2.54E-10	4.97E-10	4.70E-10
	(3.23E-10)	(5.10E-10)	(3.83E-10)	(2.25E-10)	(3.18E-10)	(3.85E-10)
N	1,582	1,582	1,582	4,196	4,196	4,196
Main controls	yes	yes	yes	yes	yes	yes

Notes: As in Table 2.4, but controlling for 1991 firm-level characteristics. Coefficients on firm size are in scientific notation, where for example E-05 denotes 10^{-5}. Firm size is defined as the number of employees.

included in the BASiD data. These characteristics are the number of employees working at a firm and its square, the share of female workers, a dummy variable capturing whether a firm employed more than 5 percent of low-skilled workers, the share of workers younger than 30, and the share older than 50. Some of the firm-level characteristics are correlated with fertility. In particular, fertility was higher among women employed in female-dominated firms or in firms with a relatively large share of low-skilled workers. Importantly, the impact of the RES demand shock on fertility is qualitatively and quantitatively robust to the inclusion of the firm-level control variables. This suggests that the potential selectivity of firms does not confound my results.

Figure A.6: Correlation between Women's RES-Values (by Industry) and Average RES-Values of their Husbands, 1991, Married Women Only



A. All industries (N=48) B. Excluding the four most extreme RES-values

Notes: Based on the Microcensus of 1991. The sample includes East German women born 1959 to 1973 who have a husband. The regression model is weighted using industry shares.

A.5.5 Assortative Mating

The previous analysis has focused on women while neglecting their spouses. This could be a problem if the labor demand shock experienced by women was correlated in systematic ways with employment prospects of their spouses. Because the East German economy was characterized by large firms, it is implausible to assume that assortative mating played no role. Drawing on the Microcensus and focusing on East German women who were married in 1991, I find that there is indeed some evidence of assortative mating. This is shown in Figure A.6, where, at the industry level, average RES-values for husbands are regressed on women's RES-values. On average, the RES measure for husbands increases by 0.12 if women's RES-values are increased by a factor of 1 (panel A).

However, microeconomic theory as well as several recent empirical studies (Lindo, 2010; Black et al., 2013; Autor et al., 2018; Schaller, 2016), suggest that negative shocks to men's labor market prospects merely operate through the income effect and hence decrease fertility. Given that these insights apply to the East German context, the presence of assortative mating should lead to an underestimation of the positive effect the RES demand shock had on the composition of mothers.

To the extent possible, I test these expectations empirically. In Table A.9 I include control variables for husbands. I again relied on the German Microcensus. Using this auxiliary data source, at the industry level I compiled the fraction of women living with a spouse in 1991. Additionally, for married women I inferred the 1991 RES-value of their spouses. At the industry-level, I then calculated the average RES-value of spouses. I merged the two industry-

Table A.9: RES and Total Number of Births/End of Period Childlessness, OLS Estimates (Cross-Sectional Regressions), Including Imputed Control Variables for Labor Market Prospects of Spouses

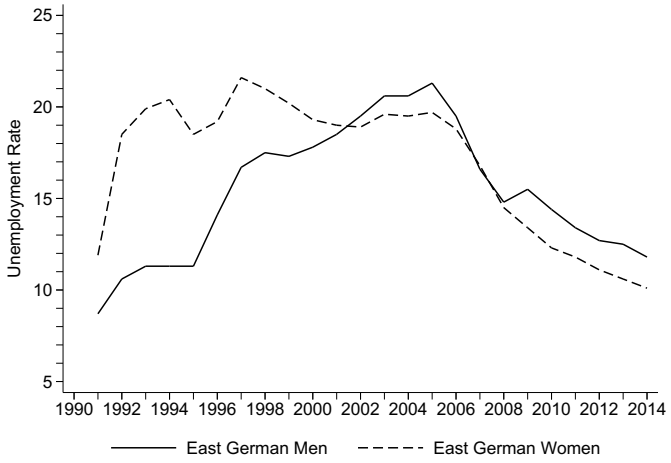
	(1) 1994	(2) 1999	(3) 2007
(a) Dep. Var: End of Period Childlessness (0/1)			
RES	-.0182	-.0458***	-.0308**
	(.0156)	(.0153)	(.0150)
P90 vs P10	-.0271	-.0683	-.0459
Mean RES-value of spouses	-.0059	.0382	.1182
	(.0855)	(.1062)	(.0972)
P90 vs P10	-.0025	.0161	.0497
Share living with spouse	-.3113**	-.1453	-.0455
	(.1458)	(.2179)	(.1891)
N	1,596	1,596	1,596
(b) Dep. Var: Total Number of Births (#)			
RES	.0131*	.0421***	.0664***
	(.0067)	(.0080)	(.0123)
P90 vs P10	.0195	.0628	.0990
Mean RES-value of spouses	.0110	-.0097	-.2655**
	(.0419)	(.0616)	(.1179)
P90 vs P10	.0046	-.0041	-.1115
Share living with spouse	.0199	-.0642	-.1151
	(.0926)	(.1254)	(.1682)
N	4,229	4,229	4,229
Main controls	yes	yes	yes

Notes: As in Table 2.4, but adding imputed control variables for husbands. These variables are based on the Scientific Use File of the 1991 Microcensus, from which women born 1959 to 1973 living in East Germany were selected. For these, the share living with a spouse as well as average RES-values of spouses (calculated for married women only) was imputed at the industry level. These two imputed variables were merged to the main sample according to a woman's initial industry.

level variables - the fraction living with a spouse in 1991 and average 1991 RES-values of spouses - with the main BASiD data.[3] These control variables were imputed for 1991 only, because partnership formation in later years could have been influenced by the labor demand shock. It turns out that average RES-values of spouses have an insignificant effect in the short-term period. In the medium- and long-term periods, point estimates suggest that average RES-values of spouses operate in the expected direction. This is tentative evidence

[3]This is inspired by Perry (2004) and Raute (2017) who similarly impute information about spouses.

Figure A.8: Unemployment Rates in East Germany, 1991-2015, By Gender



Source: Federal Employment Agency (2015). Unemployment rates refer to June 30th of a given year and are defined relative to the dependent labor force.

that indeed negative shocks to male employment prospects decrease fertility. As a result, the impact of female labor market prospects, as captured by the RES demand shock experienced by women, increases. By 2007, women at the 90th percentile of the demand shock are now estimated to have had 0.099 children more on average than their counterparts at the 10th percentile (panel (b) of Table A.9, column (3)). The effect on long-term childlessness also becomes larger in magnitude. At the end of 2007, women experiencing a more severe labor demand shock are on average 4.6 percentage points less likely to still be childless (panel (a), column (3)).

Why should male labor market prospects only impact fertility in the medium- and long-term periods? A possible reason is that East German men were most strongly hit by the labor demand shock only from the mid-1990s onwards. After 1995 and up until 2006, male unemployment rates increased to dramatic levels. By contrast, unemployment rates among East German women reached dramatic levels already in 1992 (see Figure A.8 as well as Hunt (2002) who analyzes the gender gap in employment levels post-reunification). Particularly in the medium- and long-term periods, not accounting for male employment prospects seems to lead to a downward bias of the effect of female labor market prospects on the composition of mothers.

Average RES-values are only an imperfect approximation of the labor demand shock spouses experienced. For example, this proxy variable could capture both the labor market situation of actual spouses of married women and the labor market situation of potential partners of single women. A comprehensive analysis of how men's labor market situations

Table A.10: RES and Total Number of Births/End of Period Childlessness, OLS Estimates (Cross-Sectional Regressions), Excluding Industries with Strong Employment Growth

	(1) 1994	(2) 1999	(3) 2007
(a) Dep. Var: End of Period Childlessness (0/1)			
RES	-.0647***	-.0639**	-.0175
	(.0185)	(.0260)	(.0226)
P90 vs P10	-.0983	-.0971	-.0266
N	1,557	1,557	1,557
(b) Dep. Var: Total Number of Births (#)			
RES	.0188*	.0420***	.0115
	(.0095)	(.0112)	(.0331)
P90 vs P10	.0192	.0428	.0117
N	4,135	4,135	4,135
Main contr.	yes	yes	yes
Firm contr.	yes	yes	yes

Notes: As in Table 2.4, but excluding women initially employed in one of the four industries with the strongest employment growth. After weighting, the share of women employed in these industries is 3.2 percent. I include 1991 firm-level characteristics (as in Table A.8).

impact childbearing decisions is beyond the scope of this paper. At the very least, however, the findings presented in Table A.9 cannot be reconciled with the view that the presence of assortative mating drives the positive effect the demand shock had on the composition of mothers.

A.5.6 Industries with Strong Employment Growth

Four East German industries were subject to a strong positive labor demand shock.[4] Because these industries were small in the GDR, their employment growth after reunification was pronounced. A natural question is how this impacts my results.

In Table A.10 I exclude women who were initially employed in one of these industries. The patterns of the results are similar to those presented earlier. But an important difference is that the persistence of the effect on the total number of children born until 2007 becomes smaller. It appears that for the majority of women the shock mostly impacted the timing of births. To investigate this further, in Table A.11 I include imputed control variables for

[4]These industries include the finance and insurance industries, "printing & reproduction," and "finishing trade."

Table A.11: RES and Total Number of Births, OLS Estimates (Cross-Sectional Regressions), Excluding Industries with Strong Employment Growth and Including Imputed Control Variables for Spouses

	(1) 1994	(2) 1999	(3) 2007	(4) 1994	(5) 1999	(6) 2007
Dep. Var: Total Number of Births (#)						
RES	.0013	.0402	.0721*	.0157	.0515*	.1215***
	(.0196)	(.0294)	(.0404)	(.0175)	(.0261)	(.0347)
P90 vs P10	.0013	.0410	.0735	.0160	.0525	.1239
N	4,167	4,167	4,167	4,130	4,130	4,130
Main Controls	yes	yes	yes	yes	yes	yes
Husb. Control Variables	yes	yes	yes	yes	yes	yes
Firm Control Variables	-	-	-	yes	yes	yes

Notes: As in Table 2.4, but excluding women initially employed in one of the four industries with the strongest employment growth. I include imputed control variables for spouses (as in Table A.9). In columns 4 to 6, I additionally include firm-level control variables (as in Table A.8).

spouses (see the previous section for details). Now the effect on the total number of children born becomes persistent in the long term as well.[5] Assortative mating seems to exert a relatively strong downward bias, potentially because assortative mating plays a stronger role when the industries with particularly favorable employment growth are excluded (panel (B) of Figure A.6). Therefore, the results remain robust, but the persistence of the effect on the total number of children born now depends on whether I account for assortative mating.

A.5.7 Placebo Test based on Previous Births

Finally, even though the negative labor demand shock was unexpected and exogenous, a natural concern is the potential impact of unobserved differences associated with the intensity of the labor demand shock. This would be a problem if these characteristics, and not the demand shock, caused differences in fertility. As one approach to addressing this objection, in panel (a) of Table A.12 I display the results of a placebo test. The test investigates births before the labor demand shock. The outcome variables are a dummy capturing childlessness at the end of 1990 as well as the total number of children born up until this point. When these predetermined variables are regressed on the RES measure, the coefficients are statistically insignificant. The point estimates are moreover relatively small in comparison to the mean of the outcome variables. This suggests that the labor demand shock was not associated with

[5] In this specification with husband control-variables, coefficients are imprecisely estimated (Table A.11, columns 1-3), but the precision increases with the inclusion of firm-level control variables (columns 4-6).

Table A.12: RES and Predetermined Fertility Outcomes, Placebo Test (Cross-Sectional Regressions)

	(1) Prior childlessness (0/1)	(2) Prior total no of births (#)
(a) All women born 1959-1973		
RES	-.0020	.0213
	(.0058)	(.0163)
P90 vs P10	-.0029	.0317
Mean of dep. var.	.3860	.9515
N	4,234	4,234
(b) Women born 1959-1963		
RES	-.0086	.0468
	(.0133)	(.0312)
P90 vs P10	-.0135	.0735
Mean of dep. var.	.1182	1.5250
N	1,633	1,633
(c) Women born 1964-1968		
RES	.0247**	-.0252
	(.0120)	(.0162)
P90 vs P10	.0368	-.0376
Mean of dep. var.	.3257	.9148
N	1,457	1,457
(d) Women born 1969-1973		
RES	-.0157	.0192
	(.0121)	(.0132)
P90 vs P10	-.0233	.0285
Mean of dep. var.	.0856	.1561
N	1,144	1,144
Main Controls	yes	yes

Notes: The model has the same structure as Table 2.4. The outcome variable in column (1) is a dummy variable equal to 1 whenever a women was still childless at the beginning of 1991. In column (2), the outcome is the total number of children born before 1991. Control variables for the number of children born before 1991 are obviously dropped; otherwise explanations and control variables are the same as in Table 2.4.

unobservable factors causing systematic differences in childbearing.

In panels (b) to (d) of the sample table, I repeat the placebo test but split the main sample into three different cohort groups. Again, I find that there are no systematic differences in predetermined births. In particular, point estimates for women born 1959-63 and 1964-68 have opposing signs (these cohorts are of particular interest because they were old enough such that a significant share of women had children prior to reunification). The only statistically significant effect concerns the pre-determined childlessness of women born 1964-68, but in this case, women employed in industries that were later subject to a severe labor demand shock were in fact more likely to be childless in 1990. Note also that the placebo test yields similar results if I only include main control variables, or if I additionally control for imputed variables for husbands, firm-level characteristics, and regional fixed effects (details are available from the author).

A.6 Effect Heterogeneity by Qualification Group

Table A.13: RES and Total Number of Births, OLS Estimates (Cross-Sectional Regressions), By Qualification in 1991

	(1) 1994	(2) 1999	(3) 2007	N
(a) Low qualification				
RES	.0322***	.0637***	.0695***	1,147
	(.0092)	(.0133)	(.0203)	
P90 vs P10	.0480	.0949	.1036	
(b) Medium qualification				
RES	.0073	.0285***	.0165	2,929
	(.0085)	(.007)	(.0115)	
P90 vs P10	.0109	.0425	.0246	
(c) High qualification				
RES	.0306**	.1156***	.1203***	158
	(.0131)	(.0139)	(.0145)	
P90 vs P10	.0236	.0890	.0926	
Main controls	yes	yes	yes	

Notes: Explanations are analogous to Table 2.4. I refer to women without formal qualification, with apprenticeship degrees, and to graduates, respectively. As before, "Total Number of Births" means the number of children born between 1991 and the end of 1994, 1999, or 2007. I no longer control for qualification.

B Appendix to Chapter 3: Refugee-specific Government Aid and Child Refugees' Economic Success Later in Life

B.1 Additional Tables

Table B.1: Description of Governmental Aid Programs Covered by C-Status

Category of Specific Program	Specific Rights and Entitlements	Legal Basis
Employment	Privileged access to occupations and industry branches with barriers to entry	§§ 69-71 BVFG
Employment	Acknowledgement of certified qualifications obtained before May 1945	§ 92 BVFG
Employment	Simplified procedure for replacing lost certificates	§ 93 BVFG
Employment	Possibility to be assigned a job subsidized by the government	LAG
Employment, Apprenticeship training	Support of employees through privileged treatment by employment agencies, privileged re-entry into previous occupations, privileged access to apprenticeships and to public funds supporting apprenticeship training programs, provision of cheap loans and debt guarantees used for the creation of permanent jobs for refugees	§§ 77-79 BVFG
Family reunion	Granted right to reunite with family members	§ 94 BVFG
Financial Assets	Exchange of financial assets in former German currency	Other
General support	Counseling by organizations for expellees and refugees, free of charge	§ 95 BVFG
Geographic location	Participation in geographical reallocation process	§§ 26-34 BVFG
Housing	Privileged access to rent-controlled flats built as part of public social housing programs and to flats subsidized via tax cuts, cheap loans and cash benefits	§ 80 BVFG
Housing	Access to real estate loans, in case previous real estate was lost	$ 301 LAG
Housing	Possibility to be assigned a flat that has been sponsored through a governmental program (*Wohnraumhilfe*)	LAG
Reparations	Reparations paid to victims of the Nazi regime	BEG and others
Self-employment	Cheap loans and cash benefits for tenants or buyers of agricultural businesses / farms. (West German sellers and landlords of farms and agricultural land received tax cuts if they sold/rented to individuals with C-status).	§§ 35-68 BVFG
Self-employment	Support for self-employed persons (outside agriculture) through access to loans, low interest rates, debt guarantees, co-partnerships, tax cuts and cash benefits, privileged access to public contracts, privileged access to public real estate and other property	§§ 72-76 BVFG

Description of Governmental Aid Programs Covered by C-Status, Continued

Category of Specific Program	Specific Rights and Entitlements	Legal Basis
Self-employment	Access to loans for persons who lost their economic capital; this concerns professional persons, farmers, business owners	§ 301 LAG
Self-employment	Privileges for self-employed persons through access to loans with small interest rates; other measures	AVAVG
Social Security	Same rights concerning social security and unemployment insurance as native West Germans	§ 90 BVFG
Social Security	Cash benefits/alimony payments for persons who lost their economic capital and are older than 65 (women: 60) or unable to work	§ 301 LAG
Social Security	Cash benefits to purchase household and personal effects, in case these have been lost	§ 301 LAG
Social Security	Tax cuts as part of the equalization of burdens program (*Lastenausgleichsabgaben*)	LAG
Social Security	Upgrading of old life insurance schemes	Regulations, life + pension insurance
Social Security	Years 1945 and 1946 and episode of escape from GDR count as contributory periods toward pension entitlements	BGB1 I, p. 45 & p. 88
Social Security	Compensations covered by pension insurance scheme in case escape resulted in death or inability to work	BGB1 I, p. 45 & p. 88
Social Security	Possibility for former self-employed persons to continue contributions to pension insurance in West Germany; formerly self-employed persons are granted additional pension entitlements for episodes after age 50	BGB1 I, p. 45 & p. 88
Social Security	Entitlement to unemployment insurance after two years of residence in West Germany without employment	AVAVG § 145 Abs. 1
Tax privilege	Three years of privileged tax exemption limits; purchase of household and personal effects after escape from GDR lead to tax deductions	§ 33 EStG
Tax privilege	Eligibility for certain tax cuts for self-employed persons and farmers	§§ 7a, 7e, 10a, 13 Abs. 4 EStG
Vocational training and university attendance	Education/training allowance to obtain a vocational qualification or university degree (*abgeschlossene Berufsausbildung*); in case the recipient and relatives lack the required financial means	LAG

Notes: Notes: Authors' compilation and summary based on Lüder (1957). Abbreviations can be translated as follows: BVFG - Federal Expellee Law, LAG - Equalization of Burdens Law, BEG - Federal Reparation Law, AVAVG - Law for Employment Service and Unemployment Insurance, BGB - German Civil Code, EStG - Income Tax Law. C-status was a necessary condition for eligibility; East German migrants lacking C-status were excluded from the described programs. For more details on the BVFG and the LAG see Werber et al. (1954) and Nahm (1967), respectively.

Table B.2: Additional Information on Variable Definitions and Underlying Concepts

Refugees:
Persons who migrated from East Germany to West Germany between 1946 and 1961, excluding expellees from Eastern European territories.

Qualification Variables:
Low: no formal vocational qualification; medium: completed apprenticeship training; high: graduates from universities or institutions of applied sciences, as well as engineers and technicians

Individuals' total monthly net income 1971:
"Income" in the Microcensus is defined as an individual person's net monthly total income from all income sources. This income definition provides a suitable variable for our purposes, as it captures a person's socio-economic status; but should not be confused with (labor market) wages which are not elicited in the Microcensus. The income variable is measured in seven income brackets, which reach from [1, 150 German Marks) to [1800, + infinity). We performed interval regression analysis with logged income values referring to the interval endpoints and a missing value at the censored upper end (based on men observed in West Germany; i.e., not only the GDR refugees). The dependent variables were age and its square, qualification (6 categories), economic status (13 categories; which distinguish between different categories among the self-employed, civil servants, employees, workers, and non-employed workers), and region (35 categories referring to administrative regions called *Regierungsbezirke*). Based on this approach, we predicted log income values. (We adjusted the transformation to levels such that the information is meaningful also in the top-income bracket, see Cameron and Trivedi (2010, p. 108)). Note that the income variable excludes farmers and agricultural workers.

High employment status:
Dummy variable equal to 1 whenever a person is employed as a high-level civil servants or employee or belongs to the worker elite.

Father's occupational status, list of categories included in analysis
Self-employed outside of agriculture; high-level civil servants; medium-level civil servants; low-level civil servants; high-level employees; medium-level employees; low-level employee; worker elite; qualified workers; workers with on the job and no training; farmers and helping family members; in training or school/university; non-employed

Father's industry, list of categories included in analysis
Agriculture; energy and mining; manufacturing; construction; retail; transport and information transmission; finance and insurance; hotels and restaurants; low-skilled services; science, education, art; health; qualified services; churches and other organizations; private households; public administration; missing industry.

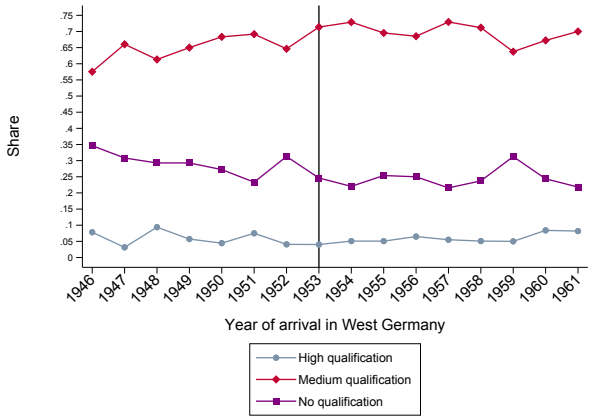
Father's and mother's qualification, list of categories included in analysis
No vocational qualification and at most a basic school education; no vocational qualification and an intermediate or higher school education; vocational qualification and at most a basic school education; vocational qualification and an intermediate or higher school education; technician, graduate from professional school (*Fachschule*); engineer, graduate from university; missing information

Table B.3: Summary Statistics for Years of Exposure (EXP)

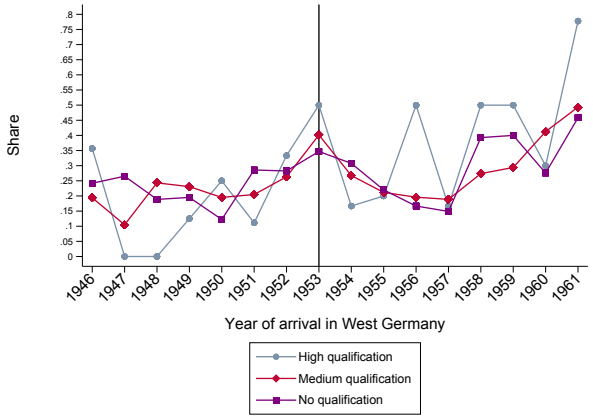
(1) Age in 1971	(2) Mean	(3) St. Dev.	(4) Min	(5) Max	(6) N
15	2.6	5	0	14	18
16	4.9	6.2	0	15	32
17	5.1	6.2	0	16	45
18	5	6.9	0	17	53
19	7.1	7.3	0	18	53
20	7.3	7.7	0	18	71
21	3.6	6.6	0	18	51
22	4.2	6.7	0	18	58
23	5.7	7.8	0	18	46
24	5.8	7.7	0	18	61
25	4.5	7.5	0	18	69
26	5.4	7.4	0	18	57
27	7	8.4	0	18	81
28	5.9	7.9	0	18	93
29	5.3	7.7	0	18	91
30	4.2	6.9	0	18	92
31	4.2	7	0	18	123
32	3.6	6.6	0	18	134
33	3.9	6.5	0	18	144
34	4.2	7	0	18	153
35	3.2	6.4	0	18	143
36	3.3	6.4	0	18	160
37	3.3	6.6	0	18	143
38	3.1	6.4	0	18	111
39	4.1	7.1	0	18	110
40	3.1	6.5	0	18	98
41	3.2	6.4	0	18	117
42	3	6.3	0	18	133
43	4.7	7.5	0	18	158
44	4	7	0	18	147
45	4	7	0	18	118
46	4.8	7.4	0	18	105
47	4.9	7.5	0	18	98
48	4.4	7.3	0	18	113
49	4.8	7.5	0	18	119
50	4.3	7	0	18	119
51	5.9	8	0	18	86

B.2 Additional Figures

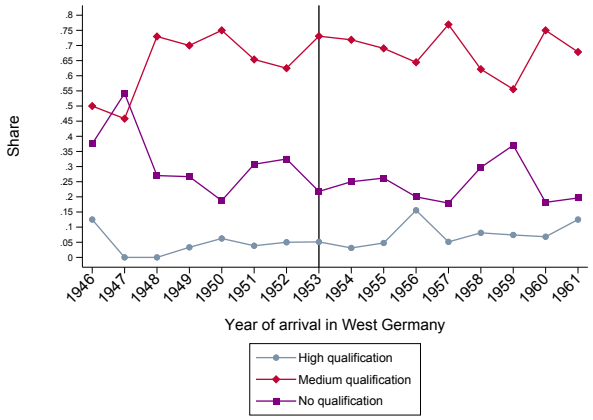
Figure B.1: Father's Educational Attainment



A. Distribution of Refugee Children and Young Adults Across Fathers' Education, by Year of Arrival



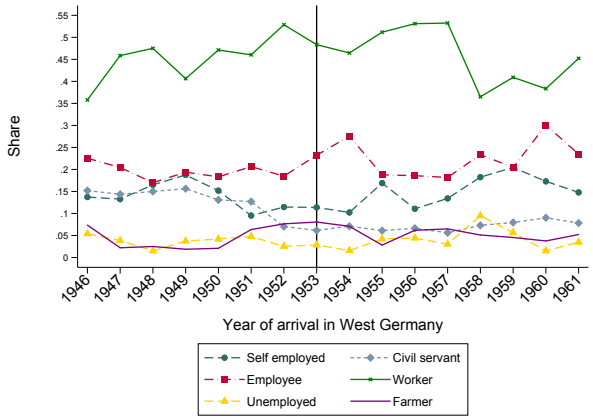
B. Share of Aid-Eligible Refugee Children and Young Adults, by Year of Arrival in West Germany and by Fathers' Education



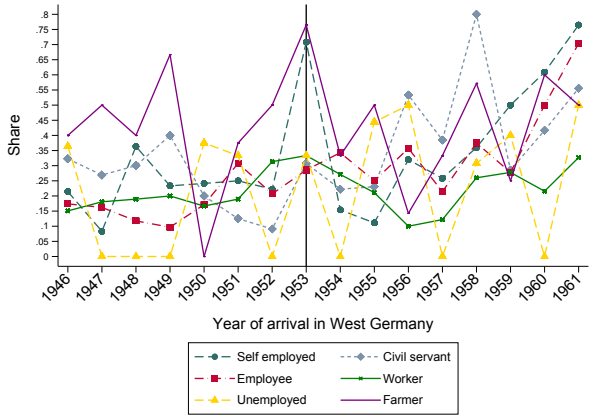
C. Distribution of Aid-Eligible Refugee Children and Young Adults Across Fathers' Education, by Year of Arrival in West Germany

Notes: *Panel (A):* This figure shows the distribution across fathers' educational attainment of refugees who arrived in West Germany at ages 1-25, by year of arrival. The values for each education class by year of arrival sum to 1. *Panel (B):* This figure shows the share of aid-eligible refugees who arrived in West Germany at ages 1-25, by year of arrival and by fathers' educational attainment. *Panel (C):* This figure shows the distribution of aid-eligible refugees who arrived in West Germany at ages 1-25 across fathers' educational attainment, by year of arrival.

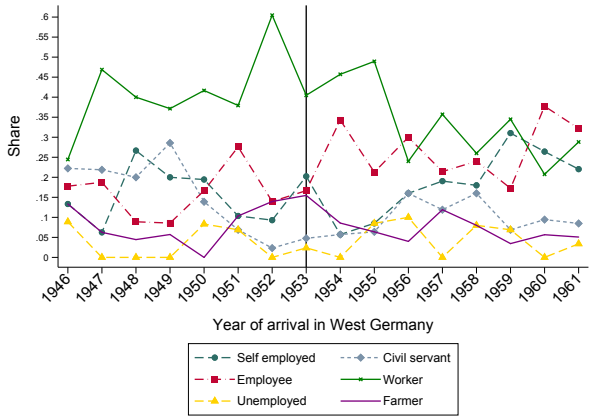
Figure B.2: Father's Employment Status



A. Distribution of Refugee Children and Young Adults Across Fathers' Employment Status, by Year of Arrival



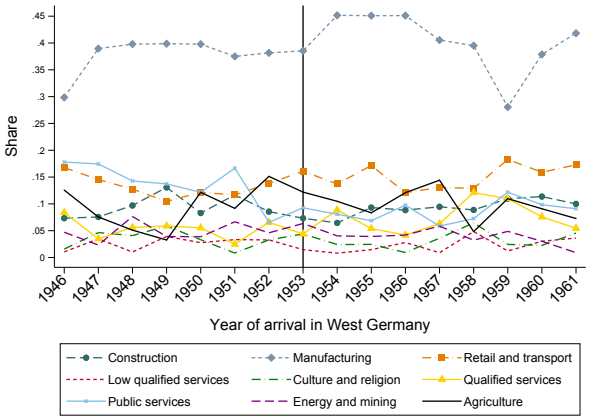
B. Share of Aid-Eligible Refugee Children and Young Adults, by Year of Arrival in West Germany and by Fathers' Employment Status



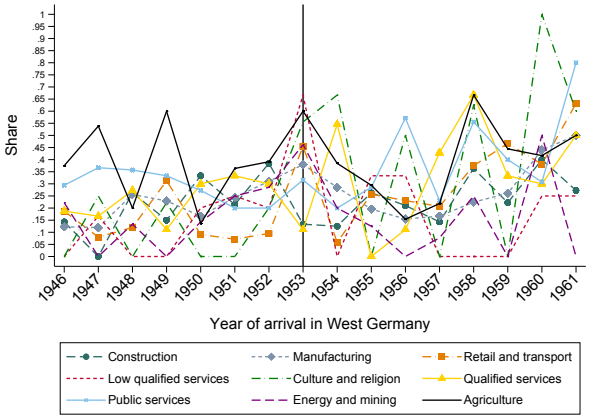
C. Distribution of Aid-Eligible Refugee Children and Young Adults Across Fathers' Employment Status, by Year of Arrival in West Germany

Notes: *Panel (A):* This figure shows the distribution across fathers' occupational status of refugees who arrived in West Germany at ages 1-25, by year of arrival. The values for each employment status group by year of arrival sum to 1. *Panel (B):* This figure shows the share of aid-eligible refugees who arrived in West Germany at ages 1-25, by year of arrival and by fathers' employment status. *Panel (C):* This figure shows the distribution of aid-eligible refugees who arrived in West Germany at ages 1-25 across fathers' employment status, by year of arrival.

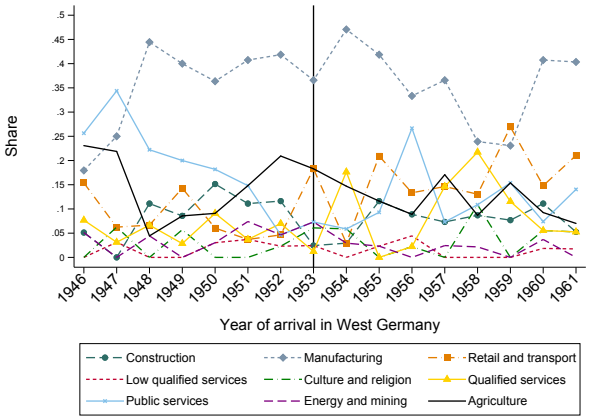
Figure B.3: Father's Industry of Employment



A. Distribution of Refugee Children and Young Adults Across Fathers' Employment Status, by Year of Arrival



B. Share of Aid-Eligible Refugee Children and Young Adults, by Year of Arrival in West Germany and by Fathers' Employment Status



C. Distribution of Aid-Eligible Refugee Children and Young Adults Across Fathers' Employment Status, by Year of Arrival in West Germany

Notes: *Panel (A):* This figure shows the distribution across fathers' industry of employment of refugees who arrived in West Germany at ages 1-25, by year of arrival. The values for each employment status group by year of arrival sum to 1. *Panel (B):* This figure shows the share of aid-eligible refugees who arrived in West Germany at ages 1-25, by year of arrival and by fathers' industry of employment. *Panel (C):* This figure shows the distribution of aid-eligible refugees who arrived in West Germany at ages 1-25 across fathers' industry of employment, by year of arrival.

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Selbstständigkeitserklärung

Ich erkläre, dass ich die vorliegende Arbeit selbstständig und nur unter Verwendung der angegebenen Literatur und Hilfsmittel angefertigt habe.

Ich bezeuge durch meine Unterschrift, dass meine Angaben über die bei der Abfassung meiner Dissertation benutzten Hilfsmittel, über die mir zuteil gewordene Hilfe sowie über frühere Begutachtungen meiner Dissertation in jeder Hinsicht der Wahrheit entsprechen.

Genf, den 11. November 2018 Hannah Liepmann